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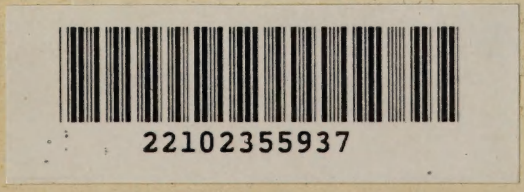
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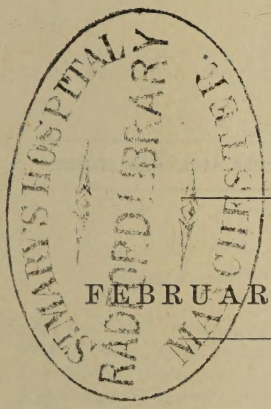

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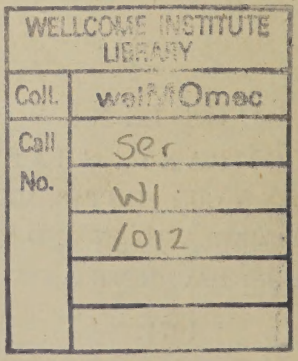

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10. Ought the Contagious Diseases' Act be repealed?

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16. A treatise on gout, rheumatism and the allied affections. By Peter Hood, M.D. London: J. and A. Churchill. 1871. 8vo, pp. 417.

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18. Lectures on obstetric operations, including the treatment of hæmorrhage, and forming a guide to the management of difficult labour. By Robert Barnes, M.D., London, F.R.C.P.; Obstetric Physician to St. Thomas's Hospital, &c., &c. Second edition, revised and extended. London: J. and A. Churchill. 1871. 8vo, pp. 508.

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NOTICES TO CORRESPONDENTS.

We have been obliged to hold over several Original Communications, Reviews, and Clinical Records.

Authors of Communications are requested to write the prescriptions in their papers in full, and in English.

Books and Periodicals published in Northern Europe and the German States intended for our Journal, should be transmitted "For the Editor of the Dublin Quarterly Journal of Medical Science, care of Messrs. Trubner and Co., *London*," *through the following Houses, viz.*:—Messrs. Schneider and Co., *Berlin*; Messrs. Braumuller and Son, *Vienna*; M. F. A. Brockhaus, *Leipzig*; *and also their Correspondents* in the principal Towns on the Continent. Our Correspondents in France, Belgium, Italy, and Spain, are requested to communicate with us through "Messrs. Hachette and Co., 77, *Boulevard St. Germain, Paris*."

AMERICAN Books and Journals often come to hand with such an amount of Charges on them, that we cannot release them. It is requested that *all* communications from the United States shall be forwarded to Mr. John Wiley, *New York*; or Mr. Henry C. Lea *Philadelphia*, directed to us, to the care of Messrs. Trubner and Co., *London*.

BOOKS RECEIVED, FEBRUARY, 1871.

1. St. Thomas's Hospital reports. New series. Vol. i. London : John Churchill and Sons. 1870.

2. On the wasting diseases of infants and children. By Eustace Smith, M.D., Lond. ; Member of the Royal College of Physicians ; Physician to his Majesty the King of the Belgians. Second edition, revised and enlarged. London : James Walton. 1870. 8vo, pp. 309.

3. The medical works of Francisco Lopez de Villalobos, the celebrated Court Physician of Spain. Now first translated, with commentary and biography. By George Gaskoin, K. Commander of the R. Military Order J. Christo of Portugal, &c. London : John Churchill and Sons. 1870. 8vo, pp. 312.

4. Report on the sanitary administration of the Panjab, 1869. Lahore : Lawrence Press. 1870.

5. Medico-chirurgical transactions. Vol. liii. London : Longmans, Green, Reader, and Dyer. 1870.

6. Contributions to surgical and general pathology. By John Chiene, M.D., F.R.C.S.E.; Teacher of Operative Surgery, Minto House, Edinburgh. 1870. Sewed.

7. On the mortality of childbed and maternity hospitals. By J. Matthews Duncan, A.M., M.D., F.R.C.P.E., F.R.S.E. ; Lecturer on Midwifery and Diseases of Women and Children in Surgeons' Hall Medical School, &c. Edinburgh : Adam and Charles Black. 1870. 8vo, pp. 172.

8. The Liverpool medical and surgical reports, October, 1870. Edited by P. M. Braidwood, M.D., and Reginald Harrison, F.R.C.S. London : John Churchill and Sons. 1870.

9. An introduction to the osteology of the mammalia. By Wm. Henry Flower, F.R.S., F.R.C.S. ; Hunterian Professor of Comparative Anatomy and Physiology, and Conservator of the Museum of the College. London : Macmillan and Co. 1870. Small 8vo, pp. 344.

10. A sketch of the life and writings of Robert Knox, the Anatomist. By his pupil and colleague, Henry Lonsdale. London : Macmillan and Co. 1870. 8vo, pp. 420.

11. A report on the microscopic objects found in cholera evacuations, &c. By Timothy Richards Lewis, M.B. : Assistant Surgeon, Her Majesty's British Forces. Printed by order of Government. Calcutta : Office of Superintendent of Government Printing. 1870.

12. Introductory address delivered at the opening of the session, 1870–71, Queen's Hospital, Bermingham. By Alexander Fleming, M.D. ; Fellow of the Royal College of Physicians, London, &c. London : John Churchill and Sons.

13. Journal of the Scottish Meteorological Society. July, 1870. William Blackwood and Sons, Edin. and Lond.

14. The American journal of syphilography and dermatology. Edited by M. H. Henry, M.D. ; Surgeon to New York Dispensary. New York : F. W. Christern.

15. Observations on the treatment of zymotic disease by the administrations of sulphites. By Professor John Polli, M.D., Milan. Sewed.

16. Della malaria e dei febrifughi. Del Professore Giovanni Polli. Sewed.

17. Prostitution in Paris : Dr. Chapman's replies to the remarks of M. le Fort and Mr. Berkeley Hill on certain passages in the articles on prostitution published in the Westminster Review. London : Trübner and Co. Sewed.

18. The causes and treatment of lateral curvature of the spine. By Richard Barwell, F.R.C.S. ; Surgeon to and Lecturer on Anatomy at the Charing Cross Hospital. Second edition. London : Macmillan and Co. 1870. Small 8vo, pp. 211.

19. The diseases of children. By Fleetwood Churchill, M.D., Dub. and Ed., M.R.I.A. ; Fellow and ex-President of the King and Queen's College of Physicians, &c. ; and Fleetwood Churchill, Junr., Fellow of the King and Queen's College of Physicians. Third edition. Dublin : Fannin and Co. 1870. Pp. 900.

20. The treatment of surgical inflammations by a new method which greatly shortens their duration. By Fourneaux Jordan, F.R.C.S., Eng. ; Surgeon to the Queen's Hospital ; Professor of Surgery at the Queen's College, Birmingham. London : John Churchill and Sons. 1870. Crown 8vo, pp. 158.

21. The change of life in health and disease : a practical treatise on the nervous and other affections incidental to women at the decline of life. By Edward John Tilt, M.D. ; Vice-President of the Obstetrical Society of London. Third edition. London : John Churchill and Sons. 1870. 8vo, pp. 296.

22. Body and mind : an inquiry into their connexion and mutual influence, specially in reference to mental disorders, being the Gulstonian lectures for 1870. By Henry Maudsley, M.D., Lond., &c. London : Macmillan and Co. 1870. Small 8vo, pp. 189.

23. Transactions of the Pathological Society of London. Vol. xxi.

24. An inquiry into the real nature of hysteria. By D. De Berdt Hovell, F.R.C.S.E., &c. Sewed.

25. Method and medicine. By Balthazar W. Foster, M.D. ; Professor of Medicine in Queen's College, Birmingham. London : John Churchill and Sons. 1870. 8vo, pp. 58.

26. Étude sur les grossesses compliquées de myomes utérines. Par Le D. Ernest J. Lambert. Paris : Louis Leclerc. 1870. 8vo, pp. 246.

27. A paper on median lithotomy. By James L. Little, M.D., of New York,

Surgeon to St. Luke's Hospital. Philadelphia: Collins. 1870. Sewed.

28. Lessons in elementary physics. By Balfour Stewart, LL.D., F.R.S., Professor of Natural Philosophy, Owen's College, Manchester. London: Macmillan and Co. 1870. Small 8vo, pp. 372.

29. Partial paralysis from reflex irritation caused by congenital phymosis and adherent prepuce. By Lewis A. Sayre, M.D., &c. Philadelphia: Collins. 1870. Sewed.

30. Harveian Oration, delivered at the Royal College of Physicians, London, June 24, 1870. By William Gull, M.D., F.R.S. London: John Churchill and Sons. Pp. 52.

31. Observations on some of the more recent methods of treating wounds, and on excision of the knee-joint. By Edward Lund, F.R.C.S., Lecturer on Anatomy, and one of the Surgeons to the Manchester Royal Infirmary. Manchester: J. E. Cornish. 1870.

32. Dr. Scoresby Jackson's note-book of materia medica. Second edition. Edited by Dr. A. MacDonald, M.A. Edinburgh: Maclachlan and Stewart. 1871. Small 8vo, pp. 686.

33. Observations on the physiological and therapeutical effects of galvanization of the sympathetic. By A. D. Rockwell, A.M., M.D., and George M. Beard, A.M., M.D. New York Printing Company. 1870. Sewed.

34. On phthisis and the supposed influence of climate. By William Thomson, L.R.C.S. Edin. Melbourne: Stillwell and Knight. 1870. 8vo. pp. 160.

35. "Prevention better than cure." An introductory lecture delivered at the Cork South Infirmary. By W. Jackson Cummins, M.D., Physician to the Hospital. Cork: T. Petty and Co. Sewed.

36. A system of surgery, theoretical and practical, in treatises by various authors. Edited by T. Holmes, M.A. Cantab. Second edition. Vol. iv. Diseases of the organs of locomotion, of innervation, of digestion, of respiration, and of the urinary organs. London: Longmans, Green, and Co. 1870. 8vo, pp. 1119.

37. Introduction to the study of inorganic chemistry. By William Allen Miller, M.D., D.C.L., LL.D., &c. London: Longmans, Green, and Co. 1871. Small 8vo, pp. 288.

38. Pocket diagram and notes of valvular heart disease. Edited by Professor Harvey, M.D., Physician, Aberdeen Royal Infirmary. Edinburgh. Maclachlan and Stewart.

39. An essay on the treatment of aneurism, with experiments for the closure of arteries by a new method. By Benjamin Howard, A.M., M.D., of New York. Philadelphia: Collins. 1870.

40. Syphilis of the nervous system. By E. L. Keyes, M.D. New York: D. Appleton and Co.

41. Lectures upon diseases of the rectum. By W. H. Van Buren, A.M., M.D. New York: D. Appleton and Co. London: Trübner and Co. 1870. Small 8vo, pp. 164.

42. On the cellular structure of the red blood corpuscles. By Joseph G. Richardson, M.D., Microscopist to the Pennsylvania Hospital. Philadelphia: Collins.

43. A handy book on Health, and how to preserve it. By Charles A. Cameron, M.D., Professor of Hygiene in the Royal College of Surgeons in Ireland. Second and revised edition. London: Cassell, Petter, and Galpin.

44. Practical lithotomy and lithotrity, or an inquiry into the best modes of removing stone from the bladder. By Sir Henry Thompson. Second edition, considerably enlarged. London: J. and A. Churchill. 1871. 8vo, pp. 327.

45. The deformities of the human body. A system of orthopædic surgery. By Bernard E. Brodhurst, F.R.C.S., Lecturer on Orthopædic Surgery at St. George's Hospital. London: J. and A. Churchill. 1871. 8vo, pp. 259.

46. On a localized outbreak of typhoid fever in Islington, traced to the use of impure milk. By Edward Ballard, M.D. (Lond.), Medical Officer of Health for Islington. London: J. and A. Churchill. 1871. Sewed.

47. On the treatment of intra-thoracic aneurism by the distal ligature. By Christopher Heath, F.R.C.S., Teacher of Operative Surgery in University College. London: J. and A. Churchill. 1871.

48. What is malaria? And why is it most intense in hot climates? By C. F. Oldham, M.R.C.P.E., Assistant-Surgeon, H. M. Indian Forces. London: H. K. Lewis. 1871. 8vo, pp. 186.

49. The discovery of the nature of the spleen. By Henry R. Sylvester, B.A., M.D. (Lond.) London: John Churchill and Sons. 1870. Fcap. 3vo, pp. 64.

50. Twelfth Annual Report of the General Board of Commissioners in Lunacy for Scotland. Edinburgh: Her Majesty's Stationery Office. 1870.

51. Lectures and essays on the science and practice of surgery. By Robert M'Donnell, M.D., F.R.S., Fellow of the Royal College of Surgeons in Ireland, one of the Surgeons to Dr. Steevens' Hospital. Part I. Clinical Lectures on Venereal Diseases. Dublin: Fannin and Co. 1871. 8vo, pp. 133.

52. Harvard University. A descriptive catalogue of the Warren Anatomical Museum. By J. B. S. Jackson, M.D. Boston: A. Williams and Co. 1870. 8vo, pp. 759.

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THE

DUBLIN QUARTERLY JOURNAL

OF

MEDICAL SCIENCE.

FEBRUARY 1, 1871.

PART I.

ORIGINAL COMMUNICATIONS

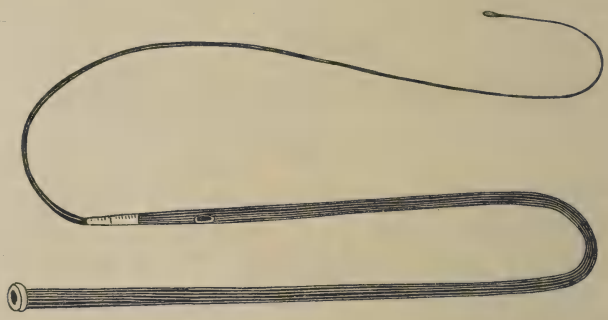
ART. I.—*Observations on the Treatment of a Hundred Cases of Stricture of the Male Urethra, by gradual and immediate Dilatation, External and Internal Urethrotomy.* By WILLIAM STOKES, jun., M.D., Surgeon to Richmond Surgical Hospital; Lecturer on Theoretical and Operative Surgery, Carmichael School of Medicine; Member of the Surgical, Pathological, and Obstetrical Societies of Ireland; Fellow of the Royal Medico-Chirurgical Society of London, &c., &c.

HAVING had, for many years, a considerable experience in the treatment of stricture of the male urethra, it may not, I hope, be considered devoid of interest and importance, if I briefly discuss the various methods of treatment I adopted in a hundred cases of that affection, noted among others in my daily surgical records, and also mention the particulars of some of those cases which presented features of special clinical interest.

The hundred cases above alluded to were treated in the following ways :—

First—Gradual dilatation.
Second—External urethrotomy.
Third—Internal urethrotomy.
Fourth—Forced dilatation.

Of these cases, seventy-six were treated by the first, four by the second, twelve by the third, and eight by the last method. It will thus be seen that the great majority of them were treated by the gradual dilatation method, and in doing so, I employed many and various instruments for the purpose, as, for example, metallic and gum-elastic catheters and bougies, and those composed of laminaria, wax, vulcanized India-rubber, papier maché, the French "bougies olivaires," and "bougies à boule," and also the combination of the elastic bougie and catheter, which I have ventured to term the double-length bougie catheter, an instrument which I have found of great service in the treatment of some difficult and complicated cases of stricture.

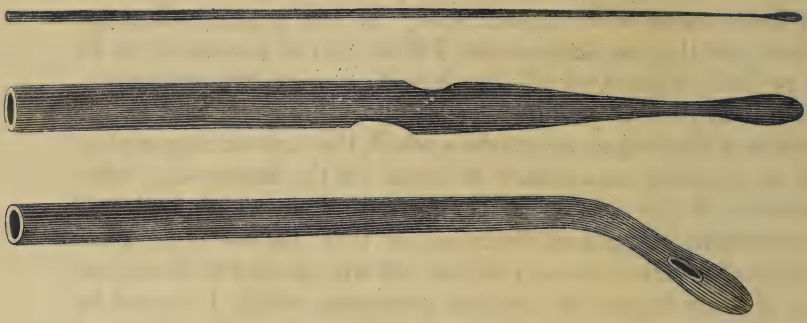


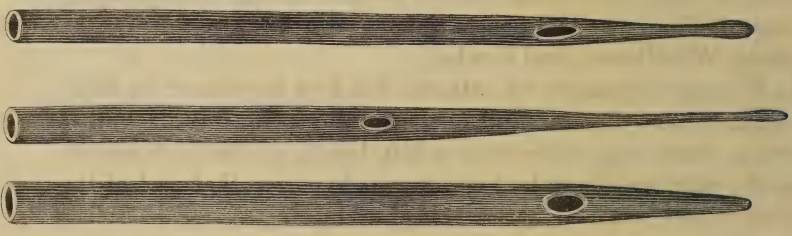
The Double-length Bougie Catheter.

But for the treatment of ordinary uncomplicated cases of stricture by gradual dilatation, I must unhesitatingly give the preference to the French "bougies olivaires," and "bougies à boule."

There are some warm advocates for the treatment of stricture by laminaria bougies, but there is one very great objection I have to them, which is, that owing to the dilatation of the laminaria beyond the seat of the stricture, the difficulty in withdrawing the instrument is, in most cases, very great, and is generally attended with considerable pain.

There can be no doubt that, although the treatment by gradual dilatation is the slowest, it is unquestionably the surest and safest method, and the one which, judiciously carried out in practised hands, is least likely to be followed by relapse. I am not one of those who hold that urethral stricture is an incurable affection, for





On Stricture of the Urethra, by Mr. William Stokes.

I have had occasion to observe some cases, though I admit they are exceptional, in which, after the lapse of years from the time of treatment, there was no sign or symptom of any re-contraction. But these cases were treated solely by the gradual dilatation method, and their non-recurrence, I think, can be accounted for by the probable absorption of the deposit, causing the contraction, which absorption is produced, or at all events facilitated by the presence of the bougies or catheters, which the surgeon may employ.

It is obviously unnecessary to dwell on the history and other particulars of the cases of urethral stricture, in which I adopted the treatment of gradual dilatation, as they did not present any features of peculiar clinical interest, but will proceed to detail some cases of truly impassable urethral strictures, which I treated by external urethrotomy, and which are, in my opinion, the only cases in which so grave and formidable an operation should be had recourse to; then discuss the cases I have treated by internal urethrotomy; and lastly, consider the treatment by the "immediate" method.

The accompanying woodcuts illustrate some of the varieties of olivary bougies, which I find most useful in the treatment by gradual dilatation, of urethral stricture.

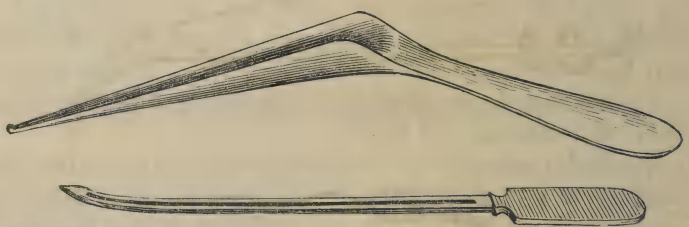
The operation of external urethrotomy is certainly one of the most difficult, if not the most difficult operation in surgery. This great difficulty arises from many causes, among which may be mentioned, the abnormal condition of the parts to be divided, the impossibility in the great majority of cases, of introducing from the wound in the perineum any guide into the bladder, and also the division of the stricture at so great a depth from the surface. Several methods have been devised for performing this operation, among which may be mentioned, the procedures of Hunter, Arnott, Guthrie, Wheelhouse, and Gouley.

In Hunter's operation "a catheter was first introduced as far as it would go, as a director, and all the sinuses were laid open to that catheter, which exposed near an inch in length of that instrument; then the catheter was in fact withdrawn, to expose that part of the urethra which was laid bare. The blood being sponged off, the orifice in the stricture was next searched for, and when found, it was dilated. The catheter was now pushed on to the bladder, although with some difficulty."

Mr. Arnott's operation consisted in cutting down on the point of a sound carried down to the stricture, and then passing a small

grooved probe through, and dividing the stricture. Mr. Guthrie recommended opening the urethra behind the stricture, and dividing the contraction by cutting from behind forwards. The full particulars of these procedures will be found in the chapter on "Perineal Section," in Sir H. Thompson's classical work on *Stricture of the Urethra.*

In Mr. Wheelhouse's operation a curved staff, grooved on its convexity, is passed down to the stricture, and the forefinger of the left hand being introduced into the rectum, a scalpel is then entered with its back to the rectum, immediately in front of the anus, and the perineum is opened by cutting upwards and forwards. The urethra is then cut into, not at the point of the instrument, but into the groove of the staff, a quarter of an inch or so above the point. Each lip of the opened urethra is then seized with a forceps, and with these the canal is drawn open. The position of the staff is then altered. It is reversed, the point turned out through the opening, and made use of to draw forwards and steady the urethra. "If the parts are now carefully sponged, I have now," Mr. Wheelhouse observes, "the interior of the canal so fully exposed to *view* that I can steadily follow its upper wall; this I do with a fine probe director, until I have cut my way completely through the strictures, and have come out into the dilated part of the urethra, which usually lies behind it, or, as sometimes happens, into a second or more strictures; this effected, I am generally able to pass the small director, which I have been using immediately onwards into the bladder."

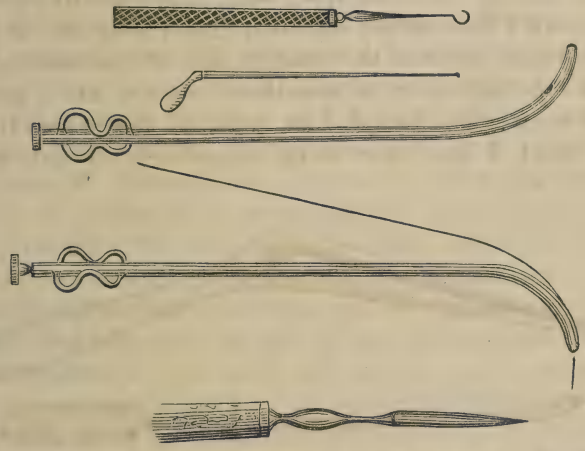


Mr. Teale's Probe-dilator, and Mr. Wheelhouse's Probe-director, for External Urethrotomy.

"Having reached the bladder with that, I turn the groove downwards; with a straight probe-pointed bistoury run along it; I make sure of the division of any further obstructing bands, and finally, I completely open up the prostatic urethra by sliding along the groove of the director the little instrument I now

show you, the 'probe-dilator,' an instrument introduced by the late Mr. Teale, for use in lithotomy, and thus obtain a broad metallic floor, upon which I can, without any danger of failure, introduce a catheter of even the largest size." [a]

Professor Gouley's operation, ingenious though it doubtless is, does not appear applicable to the kind of strictures which hitherto I have treated by external urethrotomy. They were all *truly impermeable strictures*, not allowing the introduction of the finest capillary bougie. In Mr. Gouley's procedure, a fine whalebone capillary bougie is introduced in the first instance. "The next step is to introduce a No. 8 grooved metallic catheter, with a quarter of an inch of its extremity bridged over, so as to convert the groove into a canal, the bridged portion itself being also grooved. Its introduction is accomplished by passing through the canal the fine end of the retained guide, then holding the latter steadily between the thumb and index finger of the left hand, and pushing the catheter-staff gently into the urethra, until its point comes in contact with the face of the stricture. The staff and guide are then kept in position by an assistant, who, at the same time, supports the scrotum. The



Professor Gouley's Instruments for External Perineal Urethrotomy.

patient is placed in the lithotomy position, and held by two assistants, or better, by the aid of Pritchard's anklets and wristlets." The surgeon should then make a free median incision, extending

[a] Clinical Lectures on Perineal Section, by C. G. Wheelhouse, F.R.C.S. British Medical Journal, Feb. 5, 1870.

from the base of the scrotum to within half an inch of the margin of the anus. Having exposed the urethra, the contrivance of Mr. Avery, of passing a loop of silk through each edge of the incised urethra, and each loop of silk is held by the assistant in charge of the corresponding limb,—"When the urethra is opened and the loops are secured, the catheter is withdrawn a little, so as to bring into view the black guide, then the stricture with about half an inch of the uncontracted canal behind it, is divided." The concluding step of the operation is to pass the catheter-staff, guided by the whalebone bougie, into the bladder; but should it be arrested in its course, the knife must be re-introduced, and the incision extended further back."[a]

The method which was adopted in the following cases differs in some respects from those I have above alluded to, and will be best understood by a reference to the cases.

CASE I.—*Stricture of Urethra, of sixteen years' duration, complicated with Perineal Fistula—External Urethrotomy—Recovery.*

M. H., aged sixty-two, was admitted into the Meath Hospital, under my care, on December 16, 1867, suffering from urethral stricture and perineal fistula, the latter complication having existed for upwards of six years. For some time after his admission, I made numerous but unavailing attempts to introduce an instrument into the bladder. Neither the filiform bougie of Maisonneuve, or the catgut director of Dr. Hutton's railway catheter, or the finest of the self-dilating sea-weed bougies, could possibly be introduced. On consultation with my colleagues, I determined to perform external urethrotomy. Accordingly, on the 18th January, 1868, the patient was brought into the operating theatre, and placed in the lithotomy position. A large silver catheter was introduced into the urethra down to the seat of the stricture, and held firmly in that situation by my friend, Mr. Wharton. I commenced the operation by making an incision with a long straight bistoury along the raphe of the perineum, terminating at a point about three quarters of an inch in front of the anus. The soft parts were divided freely down to the extremity of the silver instrument, which rested against the stricture. I then introduced, and with the nail of my left index finger felt distinctly the contraction beyond the point of the large silver catheter. The stricture I then

[a] On External Perineal Urethrotomy. By J. W. S. Gouley, M.D., Professor of Clinical Surgery in the University of New York, &c., &c. P. 6. New York, 1869.

succeeded in dividing with the point of the straight bistoury, which I used in the first instance for making the external incision. In doing this, which required the most delicate manipulation, I took especial care to keep the handle of the bistoury well depressed. With greater facility than I had at all anticipated, I arrived at the dilated portion of the canal, beyond the seat of the stricture. I then took a full sized gum-elastic catheter, without any stilette, which I fortunately succeeded in introducing into the bladder, through the wound. The next difficulty I had to encounter was to bring the fore part of the catheter through the anterior portion of the urethra. For accomplishing this, I had recourse to a manœuvre which Mr. Porter suggested, and which was attended with complete success, and I can recommend it most strongly to all surgeons who have to deal with similar cases. It consists in removing the ivory head of the catheter which was introduced through the wound into the bladder in the first instance; secondly, in introducing through the orifice and anterior portion of the canala conical-shaped bougie, the extremity of which, when brought down to the wound, was fixed firmly into the open end of the gum-elastic catheter. The bougie is then slowly withdrawn through the anterior portion of the urethra, bringing along with it the gum-elastic catheter. The two instruments are then separated, and the catheter fixed in the urethra in the ordinary manner. This case progressed most favourably. The wound in the perineum slowly but eventually completely closed, and when the patient left hospital, the largest sized catheter could be introduced with facility into the bladder.

Case II.—*Impermeable Urethral Stricture of ten years' duration—External Urethrotomy—Recovery.*

The following case of impermeable urethral stricture illustrates even more fully than the preceding one the great practical value of external urethrotomy.

David E., aged thirty-three, a sailor by occupation, was admitted into the Richmond Surgical Hospital, on December 18, 1869, suffering from a close stricture of the urethra. He stated that about ten years ago, while in London, he contracted gonorrhœa, of which he was never perfectly cured. A gleet was the result, which, in consequence of his irregular mode of living, and frequent exposure to wet and cold weather, became chronic. About a year after he contracted the gonorrhœa, he perceived he was unable to expel the urine in as full a stream as was natural. This state of

things continued without his seeking for any surgical advice, unti at last he was only able to void his water drop by drop.

About three months previous to his admission to the Richmond Hospital, while helping to unload a cargo of timber with which the ship in which he was employed was freighted, a heavy log of wood struck him on the testicle, which soon after swelled greatly, and the difficulty of passing water greatly increased. He then consulted a practitioner in Wales, who recommended him to come to Dublin for surgical treatment. On examining the patient, I determined the existence of a dense cartilaginous stricture, at the membranous portion of the urethra. For upwards of ten days or a fortnight, I made frequent but unavailing attempts to pass the smallest catheter, and I found that not even the introduction of the filiform bougies of M. Maisonneuve was possible. The symptoms of retention of urine at this time became so extreme, and the bladder became so enormously distended, that my clinical assistant fearing a vesical rupture, sent for me, and, after a final attempt to introduce an instrument proved unavailing, I determined to perform a perineal section.

Accordingly, at 4 p.m., on Sunday afternoon of January 2, the patient was brought into the operating theatre, and brought under the influence of chloroform, and I then proceeded to perform external perineal urethrotomy. I may say, in truth, that in consequence of the failing light, the great depth of the perineum, the violent struggles of the patient, over whom chloroform had but little effect, and the great length of the stricture, through which no instrument could be introduced for a guide, this was by far the most difficult surgical operation it has ever been my lot to perform.

It is unnecessary to go through in detail the steps of the operation I performed, which lasted fully an hour and a half, and the various but fruitless efforts I made to introduce an instrument, when I believed the stricture was sufficiently divided. The great length and depth of the stricture, and the failing light, were doubtless the chief causes of the extreme difficulty I experienced. However, my efforts were at last rewarded, and to my no slight satisfaction, I succeeded finally in introducing a large silver catheter, and drawing off an enormous quantity of fœtid urine. The instrument was then secured in the bladder, and the patient sent back to his bed, and got immediately a powerful anodyne. Five days after the operation, No. 6 gum-elastic catheter was introduced without any difficulty, and the wound in the perineum was

found to be rapidly closing. Very little urine succeeded in getting through the perineal wound. Ten days after this the perineal fistula was found to be not larger than the head of a large pin, and this I succeeded in effectually closing by a plastic operation, and on the 28th February the patient left the hospital, the largest instruments being passed without the slightest difficulty, and the perineal fistula being completely closed.

Case III.—For the particulars of the following case I am indebted to Dr. J. Alexander Ross, of the North Staffordshire Infirmary, who was my resident clinical assistant at the time the patient was under treatment in the Richmond Surgical Hospital.

Stricture of the Urethra of twelve years' duration—Retention of Urine after a fit of intoxication—Unsuccessful attempt to pass a Urethrotome Director—Relief of Retention by warm baths—Second bad attack of Retention—No way of affording relief except by puncturing the bladder or by an external Perineal Urethrotomy—Performance of the latter operation—Unfavourable termination of the case.

Michael R., aged thirty-four, was admitted into the Richmond Surgical Hospital on June 6th, 1868. About eleven or twelve years previously he contracted a gonorrhœa, which he neglected, and consequently a gleety discharge continued for eight months. Some months afterwards he found the stream of water becoming gradually smaller, until he was compelled to undergo treatment, which consisted in gradual dilatation. On the day of his admission, while in a state of intoxication, he came into hospital, suffering from retention of urine; with difficulty a small gum-elastic catheter (No. $\frac{1}{2}$) was introduced. On the 8th of June an attempt was made to introduce a No. 3 catheter, which was not successful. I then succeeded in passing a fine filiform bougie, and using this as a guide, endeavoured to introduce the groove director of Maisonneuve's urethrotome, but, owing to the tightness of the stricture, this could not be accomplished.

After this the patient had a rigor, and was only able to pass water while in a warm bath, but there was not more tenderness than usual about the urethra. Towards night he was comparatively well, except for the retention of urine, for which the warm bath alone gave relief.

June 9.—This day there was no improvement, the retention

continued, and the bladder became very full, and he complained of some tenderness in the urethra.

June 10.—The retention still continued, yet he always was able to pass a little water when in the bath. There was great pain and tenderness in the urethra, where a funnel-shaped tumour now made its appearance. The scrotum was red and ecchymosed. A small catheter (No. 2) was introduced, and cold lotion applied to the scrotum.

June 11.—The introduction of the catheter gave great relief, and the lotion apparently reduced the scrotal swelling. The catheter was left in all day and the urine allowed to flow constantly.

June 12.—The patient pulled out the catheter during the night, and this morning the urine had extravasated up to about one inch above the pelvis—a red line mapping out the extent. The funnel-shaped tumour was now well marked, and there was a tumour about one inch broad occupying the median line of the scrotum. It was now quite obvious that one of two things should be done, either to puncture the bladder above the pelvis or through the rectum, or to perform an external perineal urethrotomy. After much anxious consideration I determined on the latter operation, which I accordingly performed, much in the same manner as in the preceding case. A large gum-elastic instrument was then successfully introduced, and a large quantity of urine drawn off. The patient was then ordered bark ammonia and wine to be freely administered, and a large poultice was applied to the abdomen. The patient continued to be very heavy and dull, and his mind occasionally wandered.

On the 13th he became somewhat better. Two incisions were made at the back of the scrotum. Bowels very free; pulse 150.

On the 14th he had a rigor; the red boundary line advanced a little, yet he appeared better. Pulse 112. On the 15th two incisions were made in the abdomen. Pulse 120. Pil. plumb. c. opi. ordered to be given every third hour until the diarrhœa was checked. Three pills sufficed. Poultices and stimulants to be continued. Perineal wounds to be dressed with sol. of chloride of lime.

June 16.—Catheter obstructed. Rigors in the morning. Extravasation had extended along the abdomen; he became very thirsty; the breathing became quick and laboured. The catheter was removed and a fresh one inserted. The perineal wounds to be dressed with the tinct. benzoin co.

June 17.—He had an uneasy night, although he had two anodyne draughts, each containing chlorodyne xxx. The extravasation did not extend, the scrotal wounds sloughed, also a spot over the left tuber ischii. The integuments over the trochanter had commenced to slough. A light coloured fluid continually oozed from the abdominal wounds.

June 18.—Fast failing; his pulse rose to 140, and as the night wore on he gradually got worse, and sank at 5 a.m.

Case IV.—*Impermeable Stricture of the Urethra of ten years' standing—Frequent but unavailing attempts to pass the smallest Catheter or Bougie—External Urethrotomy—Recovery.*

G. R., aged forty, by occupation a slate shipper, a native of North Wales, was admitted into the Richmond Surgical Hospital, under my care, on the 30th June, 1870, having been recommended to me by Dr. Roberts, of Port Maddoc, N. W. The patient stated that he had been suffering for years from a close stricture of the urethra, which he ascribed to a gonorrhœa which he had much neglected. Both previous and subsequent to his admission into hospital numerous attempts were made to introduce a catheter, and after many fruitless attempts, I succeeded in passing a No. 1 catheter, which, however, escaped during the night, and though I made frequent attempts, I could not succeed in passing it again. One great difficulty I had in attempting catheterization in this case was the great tendency the patient had to faint after any prolonged attempts to introduce an instrument. It soon became obvious that external perineal urethrotomy offered the only means of introducing an instrument into the bladder, so I accordingly recommended the operation, and the patient willingly acceded to it.

On Thursday, July 21st, I performed the operation in the same manner as described in the preceding cases. The operation was, as usual, most difficult and protracted; but after a lengthened and tedious dissection with a straight triangular bistoury, the back of the instrument being kept throughout towards the rectum, and the division of the urethral contraction being made from behind forwards. After many attempts a catheter (No. 9) was introduced from the wound into the bladder, and the other extremity of the catheter brought through the anterior portion of the urethra in the manner described in the first case.

As regards the subsequent progress of the case I may mention that the healing of the wound in the perineum was very protracted,

but that ultimately it completely united. For upwards of eight weeks large-sized instruments were kept continually in the bladder without causing the slightest annoyance or irritation of any kind. After this the patient was enabled to pass, without any difficulty, one of the largest of the flexible gum-elastic French catheters. The patient then, after being under treatment for over three months, returned in health and spirits to his native country.

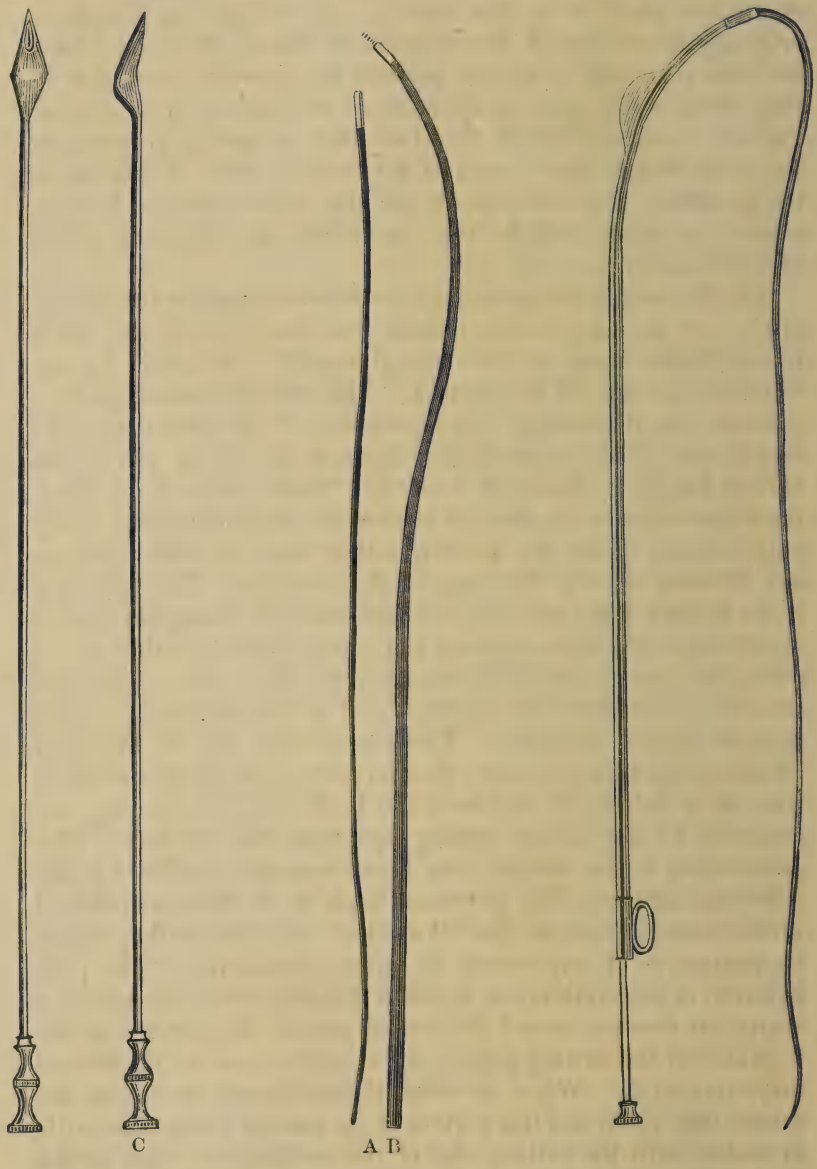
My friend and former colleague, Mr. Porter, Surgeon to the Queen, kindly assisted me during the performance of the operations just detailed, and I may here be permitted to express my warm thanks to him for the signal service he so willingly rendered on these, and many other occasions.

To estimate the surgical value of this most important and difficult operation, it is necessary, as Professor Gouley has pointed out in his able paper on this subject, to consider what are the circumstances which render the operation justifiable, the dangers which attend it, and lastly, how far it may be entitled to be considered as a cure. As regards the first of these it should, I think, be answered in this way: that it is only justifiable in those—now happily rare—cases, where the introduction of any instrument is wholly impossible. Looking at the gravity and difficulty of this procedure, I feel satisfied of this. As regards the danger Professor Gross truly remarks:—"The operation is by no means free from danger, and requires the most consummate skill for its successful execution. In truth, it should only be performed as a last and only resort for a desperate condition of things." Lastly, with reference to the third consideration as to how far, namely, the operation is to be considered as a means of cure, I may here express my conviction that no matter how skilfully and successfully the operation may have been performed, the surgeon cannot conscientiously assure his patient, subsequently, that the occasional use of a bougie will not be necessary.

I may now proceed to discuss the treatment of urethral stricture by internal urethrotomy.

INTERNAL URETHROTOMY.

There are, as is well known, many ways in which this operation is performed, the majority of which procedures are described more or less fully in most of the works which have been published on the subject of urethral stricture. In none of these published in Great Britain, so far as I am aware, is there any, even a brief



M. Maisonneuve's Instrument for Internal Urethrotomy.

description of the method of internal urethrotomy, which I introduced into practice in this country, and which was, I believe, originally devised by M. Maisonneuve, of Paris. Although I have for some years had occasion to perform the operation from time to time, many of my professional friends are still unaware of the steps of the procedure, and I feel, therefore, that no apology is necessary for giving, before the records of my cases, a brief description of the operation. The instruments for the performance of Maisonneuve's operation will be best understood by reference to the annexed woodcuts.

The first step in the operation is the introduction into the bladder of the very fine and delicate filiform gum-elastic bougie (A) which is not thicker than the double-lengthened cat-gut guide for the "railway catheter" of Dr. Hutton. This being introduced past the stricture into the bladder, the second step of the operation is the attachment of the grooved steel director (B) to the end of the filiform bougie, by means of a screw. When this is firmly fixed, the introduction of the director commences, the bougie being slowly pushed before it into the bladder, within which it coils. Seldom any difficulty attends this stage of the operation. The third step is the division of the stricture, to effect which the triangular-shaped urethrotome (C) is passed along the groove, situated either on the concavity or convexity of the steel director (B), in doing which the stricture is divided, but no other part of the urethra is in the slightest degree wounded. This constitutes one of the chief advantages of this operation; that no part of the urethra, either in front of, or behind the stricture, can be divided, an advantage not possessed by any other cutting operation for stricture. This preservation of the urethra from being wounded is effected in the following manner:—The projecting angle of the triangular-shaped urethrotome is blunt, so that its contact with the urethra cannot be productive of any wound or injury whatsoever. The part, however, of the urethrotome between the angle and its extremity is sharp, but does not wound the normal part of the urethra, as this is guided off the cutting portion of the urethrotome by the blunted projecting angle. When, however, the stricture is arrived at, this cannot take place, and this portion of the passage comes necessarily in contact with the cutting edge of the urethrotome which divides it; but no portion of the urethra, either in front of, or behind the stricture, can be wounded. When the division of the stricture is completed, the instruments are at once withdrawn, and a large-

sized catheter is introduced. The deviser of the operation has recommended for this purpose catheters made of vulcanized Indian rubber, which, from their great pliability and softness, are very grateful to the patient; but I have found that in some cases their softness and pliability renders their introduction a matter of some difficulty. Although it is desirable, after the operation, that an instrument should be introduced, it by no means follows that the introduction of one is absolutely essential to the success of the operation. In one of the cases that occurred in my hospital practice, after the division of the stricture, I could not succeed in getting in an instrument, and the patient was consequently left without one in the urethra, and nothing could have been more gratifying than the result of this case. On the eighth day after the operation the patient, in the presence of a large class, passed water in a full, strong, and continuous stream, after which I passed No. 10 catheter with the greatest ease. A similar circumstance occurred in a case that was operated on in the Richmond Hospital, by Mr. Fleming, and it ultimately did well. It is, however, much better that an instrument should, if possible, be introduced.

I may now proceed to detail, as briefly as possible, the particulars of twelve cases of urethral stricture, which I have operated on by this method.

CASE I.—*Stricture of the Urethra of twelve years' standing, from a fall on the Perineum—Internal Urethrotomy—Recovery.*

William G., aged thirty-seven, a painter by trade, was admitted into the Meath Hospital under my care, on October 5, 1864. The patient stated that eighteen years ago he fell on a bar on his perineum, and about five years after that he first observed a difficulty in passing water. The case had previously been treated by gradual dilatation, by sea-weed bougies, with such success that at the time the patient left the hospital, No. 9 catheter could be passed with facility. Since that time, however, the stricture gradually returned, and at the time of his second admission into hospital, No. 2 catheter was the largest instrument that could be introduced. I then performed the operation in the usual way, and after its completion attempted to introduce a large-sized instrument, but without success. I then left the patient in bed without any instrument being in the bladder. On the 6th day after the operation, I again attempted to introduce an instrument, and succeeded with facility in passing No. 10 catheter. The patient then left the hospital.

Case II.—*Stricture of the Urethra, of eight years' standing—Internal Urethrotomy—Recovery.*

Thomas M., aged fifty, was admitted into the Meath Hospital on September 16, 1864. The stricture, situated in the membranous portion of the urethra, was of eight years' standing. The etiology of it was involved in some obscurity, as there was no evidence of the urethra having been injured in any way, or of the patient ever having suffered from gonorrhœa. No. 3 catheter was, with considerable difficulty, introduced on the day of his admission into hospital.

On September 18, I operated in a similar manner as in the first case I have alluded to, and, immediately after the operation, introduced one of the large vulcanized Indian-rubber catheters, the size of which corresponded to No. 10 of the ordinary gum-elastic catheter. This instrument was kept in the bladder for three days, and then withdrawn. Nothing could have been more perfectly satisfactory than the way this case progressed, for, after the operation, there was no hemorrhage, no pain, and no evidence whatever of any constitutional disturbance. On the 25th September I was enabled to pass No. 12 gum-elastic catheter with the greatest ease, and the patient then left the hospital.

Case III.—*Stricture of the Urethra of twelve years' duration—Internal Urethrotomy—Recovery.*

W. B., aged forty-two, was admitted into the Meath Hospital on October 20, 1865. He stated that he had suffered from urethral stricture for upwards of twelve years, and ascribed the disease to repeated attacks of gonorrhœa, which he had had during his youth. For many years previous to his admission into hospital, he was in the habit of having instruments passed from time to time. Of late considerable difficulty was experienced in doing so, and when he came under my observation in the Meath Hospital, No 3 catheter was the largest instrument that could be introduced, and that by very delicate manipulation. On October 28, I performed internal urethrotomy; in this case dividing the stricture on the under surface of the urethra. After the operation a large conical-shaped gum-elastic catheter was introduced without any difficulty, and kept in the bladder for forty-eight hours. During the convalescence the patient had no rigor or other evidence of constitutional disturbance. On November 5, the patient left the hospital able to pass water in a full and uninterrupted stream.

CASE IV.—*Complicated Double Stricture of the Urethra of fourteen years' duration—Scrotal Fistula communicating with Urethra—Internal Urethrotomy—Recovery.*

W. K., aged fifty-two, formerly a soldier in the Bengal Army of the East India Company, was admitted into the Richmond Hospital under my care, on January 28, 1868. The patient stated that fourteen years previously he contracted gonorrhœa in India, and was treated solely by injections of sulphate of copper, since which he has suffered more or less from difficulty in passing water. About eight months ago the patient states an abscess formed in the mesian line of the scrotum, at its most depending portion, and since this opened, the urine has made its way through it forming, a urinary scrotal fistula. On examining the urethra I found, situated in the anterior portion of the canal, about three inches from its orifice, a hard callous stricture, through which, although I made repeated attempts, I could not succeed in introducing the smallest gum-elastic catheter. As all attempts to heal the fistula would necessarily prove unavailing, unless the stricture were in the first instance successfully treated, I determined that, should I be able to introduce one of the filiform bougies of Maisonneuve, to divide the stricture by internal urethrotomy. I fortunately succeeded in passing the bougie, and in doing so determined the existence of a second stricture in the usual situation. The patient did not lose more than a few drops of blood during or after the operation, and after this was completed, I succeeded in introducing without difficulty a No. 9 gum-elastic catheter, and drew off a large quantity of urine. After the operation no unfavourable symptom supervened, and eventually the patient completely recovered.

CASE V.—*Stricture of the Urethra of two years' duration—Internal Urethrotomy—Recovery.*

C. M., aged thirty-four, was admitted into the Meath Hospital under my care, on October 4, 1867, suffering from stricture of the urethra, which he stated he had had for the last two years. The stricture was situated in the membraneous portion of the urethra, and the disease the patient attributed to an attack of gonorrhœa which he had three years previously, and which he had much neglected. As this was in every respect an uncomplicated case, but so completely close a stricture as not to permit of gradual dilatation by the ordinary gum-elastic catheter, I determined to treat it by

internal urethrotomy, and accordingly performed the operation, and without any difficulty, on the 8th of October. After the operation I introduced a No. 10 catheter. No rigor, or any evidence of constitutional disturbance supervened during the patient's convalescence, and when he left the hospital so large an instrument as No. 12 could be introduced without any difficulty whatsoever.

Case VI.—*Stricture of the Urethra of fifteen years' duration—Internal Urethrotomy—Recovery.*

T. N., aged fifty-four, was admitted into the Meath Hospital under my care, on December 6, 1867, with all the symptoms of urethral stricture. The stream of urine was extremely small, and the desire for micturition so frequent as to make me suspect the case was complicated with some prostatic irritation. The stricture was an extremely close one, so much so, that it was not until I made frequent attempts, that I at last succeeded in passing a No. 2 catheter. Any larger instrument than this I failed in getting in. Accordingly, I determined to treat the case by internal urethrotomy, which operation I performed on the 9th December, and immediately after I introduced a No. 10 catheter, which I fixed in the bladder in the ordinary manner. Nothing untoward occurred during the patient's convalescence, and on the day he left hospital, December 17th, No. 11 gum-elastic catheter was passed without the slightest difficulty.

Case VII.—*Stricture of the Urethra of thirteen years' duration—Previously treated twice by the "Immediate Dilatation" method—Internal Urethrotomy—Recovery.*

Michael B., aged forty-three, was admitted into the Richmond Hospital under my care, on May 22, 1868, suffering from a very tight stricture of the urethra, situated in the region of the bulb, and which with great difficulty would only admit No. 1 catheter. The patient stated that in 1865 he had been treated by the "immediate method," and that the stricture having returned, the operation was performed a second time by another surgeon in July, 1867. The stricture had a second time recurred, and the difficulty in passing water was considerably greater than it had ever been.

On May 23, I performed internal urethrotomy, and immediately after a No. 10 gum-elastic catheter was introduced.

No rigor or other evidence of any constitutional disturbance occurred during the patient's convalescence.

On the 27th, the patient left hospital.

On September 3rd, No. 9 catheter was introduced without any difficulty.

The patient has been perfectly well ever since the operation.

CASE VIII.—*Stricture of the Urethra of ten years' duration—Internal Urethrotomy—Recovery.*

Cornelius M., aged thirty-two, was admitted into the Richmond Hospital on the 10th of last March. The case presented all the symptoms of urethral stricture, which the patient attributed to gonorrhœa, and which the patient stated he contracted about eleven years previously. The stricture, situated in the membranous portion of the urethra, was so close a one that it was not until after repeated trials that a No. 2 catheter could be passed; when this was done, I then passed in the filiform bougie of the urethrotome, and completed the operation then in the ordinary manner. A large gum-elastic instrument was immediately introduced after the operation.

Nothing untoward occurred during the convalescence of the patient, and more than four months after the operation, No. 9 catheter could be introduced with the greatest facility.

CASE IX.—*Stricture of the Urethra—Internal Urethrotomy—Recovery.*

Peter S., aged twenty-two, was admitted into the Richmond Hospital under my care, on the 20th March, 1868, suffering from stricture of the urethra, which he had, he stated, for the last two years. He attributed it to an attack of gonorrhœa, which had been treated by injections, and which he did not believe had ever been quite cured.

For some days I treated the case by partially dilating the stricture by wax bougies, as the ordinary small sized gum-elastic catheters could not be introduced. As soon as the filiform bougie of the urethrotome could be passed, the remaining steps of the operation were completed in the ordinary manner. After the operation No. 10 gum-elastic catheter was introduced.

Four hours after the operation the patient had a rigor, but no other evidence of any constitutional disturbance supervened during the convalescence of the patient. A week after the operation the patient left hospital able to pass water in a full and uninterrupted stream.

Case X.—*Stricture of the Urethra, of four years' standing—Internal Urethrotomy—Recovery,*

Peter G., aged forty-five, was admitted into the Richmond Hospital under my care, on the 6th April, 1868. The stricture, situated in the membranous portion of the urethra, was of four years' standing. The patient stated he never had any gonorrhœa, nor had he ever received any injury to the perineum. There was some obscurity, therefore, as to the cause of the stricture. The gradual dilatation method had been tried, only however for the stricture to return in a still more contracted state. The stricture was situated in the membranous portion of the urethra. At the time of the operation No. 2 gum-elastic catheter could alone be introduced, and that with much difficulty, and requiring very delicate manipulation. The operation was performed in a similar manner, as in the preceding cases, and immediately after No. 9 gum-elastic catheter was introduced. There were no rigors after the operation. Twenty-four hours after the operation the instrument was withdrawn, and no instrument re-introduced for four days. No. 10 catheter was then passed without any difficulty. The patient then left the hospital.

Case XI.—*Stricture of the Urethra, of fourteen years' duration—Internal Urethrotomy performed twice; once in* 1867, *and again in* 1870—*Successful result.*[a]

Thomas Lawlor, aged sixty, by occupation a cabinetmaker, was admitted into the Richmond Surgical Hospital under my care, on July 14, 1870. He states that fourteen years ago he contracted a gonorrhœa, which subsequently became a gleet. This was followed by urethral stricture. In 1867 the patient was under my care in the Meath Hospital, and while there he underwent the operation of internal urethrotomy. At that time the patient was in hospital for three weeks. No constitutional disturbance followed the operation, and when the patient left hospital the largest sized instrument could be introduced with facility, and he was able to pass water in a full and continuous stream. He remained in this condition for eighteen months, but quite neglected ever to follow the advice he got to have an instrument passed occasionally. At this time he led for some weeks a very intemperate life, and he noticed shortly after this the urine coming in a small stream, and

[a] From Notes taken by Mr. Agmon Vesey.

the micturition was attended with considerable difficulty. This increased up to the time of his second admission into hospital under my care, in July, 1870.

The following notes of the case were taken by Mr. Agmon Vesey:—When admitted into the Richmond Hospital, under Mr. Stokes' care, he suffered from great irritability of the bladder, and was called often during the night to pass water, which either came drop by drop, or in a very minute and twisted stream, and even sometimes stopped altogether. I failed to pass No. 1 catheter.

July 15.—Pulse very quick; tongue tremulous; ordered purgatives and baths.

July 19.—Seized with retention of urine; neither catheter or bougie could be introduced; ordered opiates, and hip-bath.

July 20.—Got great relief from the bath, and succeeded in passing water in it; had to leave hospital on account of some family affairs.

August 11.—Re-admitted.

August 17.—This morning he was operated on again by Mr. Stokes. The operation was performed without difficulty, and scarcely any blood was lost. No. 9 catheter was passed at once, and fixed in the bladder. Ordered quinine and opium.

August 18.—Everything going on well; no rigor or thirst; pulse quiet; no pain except when he passes urine, and then it pains him but slightly; has taken the mixture of yesterday regularly, and is to continue it.

August 21.—No bad symptom since; catheter still retained in the bladder.

August 23.—Catheter removed to-day by Mr. Stokes. Urine comes in a large stream; pain very trifling. To continue the quinine and opium mixture.

August 25.—Left testicle swelled and painful.

August 26.—Mr. Stokes ordered him a suspensory bandage, and poppy head stupes. The urine comes in a large stream, and without any scalding.

August 30.—The inflammation gone down greatly in the testicle. Patient very weak, and with an intermittent pulse; ordered aromatic iron mixture.

September 7.—The testicle is still inflamed, and is very indurated and tender to the touch. The skin covering it is of a dark bluish colour. Mr. Stokes thinks matter is forming.

September 17.—An incision was made by Mr. Stokes, and a large quantity of matter evacuated.

October 7.—Patient getting strong and well. The testicle regaining its usual dimensions. The urine continues to come in a full stream; a No. 7 catheter was introduced without the slightest difficulty into the bladder to-day.

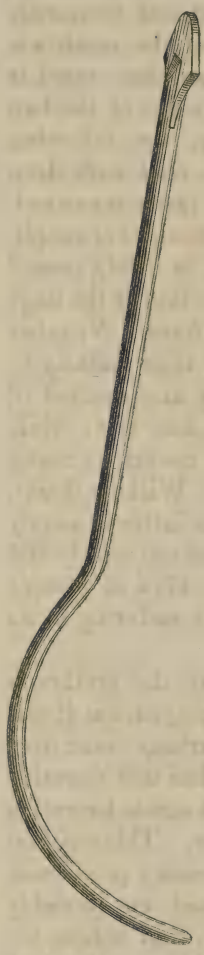
Case XII.—*Organic Stricture of the Urethra, of upwards of thirty-four years' duration—Internal Urethrotomy—Recovery.*[a]

Joseph H., aged fifty-five, by occupation a jockey, was admitted into the Richmond Surgical Hospital under my care, on the 17th July, 1868, suffering from organic stricture of the urethra. He states that thirty-five years ago he was riding a race, and while crossing a fence he was thrown forward on the pummel of his saddle. This injury produced a rupture of the urethra, which was followed by extravasation of urine. Numerous incisions had to be made in the scrotum and perineum. In consequence of this injury he was laid up for six months, and at the end of that time he found he had a stricture, and could only pass water in a very small stream. The stricture was treated at that time by gradual dilatation. He then resumed his former occupation, and after some time the stricture again got bad, but did not get so bad as to necessitate his seeking for surgical assistance until shortly before he came into the Richmond Hospital. About a week before his admission, it got worse than it had ever been before, and he suffered also greatly from a constant dribbling of the urine, accompanied with great pain in the glans penis.

At the time of his admission, the stricture, which was situated at the region of the bulb, would admit of neither catheter or bougie, and it was not until after numerous attempts were made that I, at last, succeeded in introducing a fine cat-gut bougie. This was introduced on three occasions, and left in each time for a period of two hours. I found then little difficulty in introducing the filiform bougie and grooved director of the urethrotome, and then I completed the operation of internal urethrotomy without experiencing any difficulty; and after the division of the stricture, I introduced a large vulcanized India-rubber catheter, such as M. Maisonneuve has specially recommended for the purpose. The patient rapidly recovered without any evidence whatsoever of any

[a] Case reported by Mr. Gerard Irvine.

constitutional disturbance. He then left the hospital quite able to pass water without any pain, and in a full and continuous stream.



In connexion with this operation, the following points are of considerable practical importance for the surgeon to bear in mind:

1. The operation should not be performed unless the grooved metallic director can be introduced with facility. Any forcing of this portion of the instrument into the bladder will, in the great majority of cases, be followed by rigors and other symptoms of constitutional disturbance.

2. The patient's bowels should be cleared by an enema on the morning of the operation.

3. Immediately after the operation, a full opiate with quinine should be given.

4. The catheter introduced after the operation should not be allowed to remain longer than twenty-four hours. If it is, it will probably give rise to irritation, and the formation of an abscess when the stricture is divided.

5. No instrument should be re-introduced for at least three days.

6. The patient should be kept on milk diet for forty-eight hours after the operation, and no stimulants given.

7. For some weeks after the operation, full-sized bougies or catheters, metallic or gum-elastic, should occasionally be introduced. M. Maisonneuve employs metallic bougies of a peculiar form, of which the accompanying woodcut gives a good idea. There can be no doubt that the introduction of this form of bougie into the bladder can be effected with great facility.

With reference to the "immediate method," or method by rupture, I may mention that I adopted this procedure in eight of the hundred cases alluded to at the commencement of this record, and the results obtained were not such as to make me a very warm advocate for the operation. In three of the cases very violent constitutional symptoms supervened, and in all, with two exceptions, there was rapid recontraction. I cannot, therefore, avoid coming

to the conclusion that of the two operations, rupture and internal urethrotomy, the latter operation is to be certainly preferred, as, in my experience at least, subsequent contraction less frequently occurs. In one operation the stricture portion of the urethra is divided by a single clean incision, and in the other the wound is necessarily a *lacerated* one. In estimating the merits of the two operations of rupture and internal urethrotomy, the following remarks of Sir Henry Thompson cannot but be read with deep interest, coming, as they do, from a surgeon so justly renowned. Speaking of rupture, he observes—"It possesses the merit of simplicity, for provided the first instrument, a guide, is safely passed through the stricture into the bladder, the introduction of the large tube which effects the rupture is only a matter of force. No other operation for stricture is so easily accomplished, or is so unlikely to fail in its performance. To divide a stricture by any method of incision requires much more knowledge, practice, and tact; while it is, I believe, a more perfect proceeding in its results for really hard, old, and contractible strictures than rupture. Without doubt, urethrotomy, whether external or internal (and the latter is mainly referred to here), is infinitely more difficult to perform neatly and completely." . . . . . "I am disposed to think at present that a well performed internal urethrotomy is more enduring in its results than any other operative proceeding."[a]

It is held by some surgeons that no wound in the urethra is made when the bursting or rupture method is employed, but if this were the case, I would ask, where does the hemorrhage come from that is always present to a greater or less extent when this operation is *effectively* performed? That it comes from the forcible laceration of the urethral contraction, there can be little doubt. This constant occurrence affords sufficient evidence of the necessary production of a lacerated wound at the seat of the stricture, and one probably extending to a considerable distance in front of, and behind the situation of the contraction, and I cannot but think that subsequent contraction will take place more slowly in the case where the stricture is incised than in the one where it is forcibly lacerated and torn. The two operations, also, are obviously equally "immediate."[b]

[a] The Pathology and Treatment of Stricture of the Urethra and Urinary Fistula. By Sir Henry Thompson, F.R.C.S. Third Edition. P. 175.

[b] For the performance of the Operation of Immediate Dilatation, Mr. Richardson's Modification of Perève, or Holt's Dilator, is, I think, the best. I have also used Mr. Smyly's modification with good effect.

In conclusion, I may state what I believe to be the principles that should guide the surgeon in the treatment of urethral stricture. First, that all cases in which it is possible to introduce a catheter or bougie, should be treated by the method of gradual dilatation, as, in skilful hands, it is the safest, and the most effectual. Secondly, that strictures which are impermeable to catheters should be treated by internal urethrotomy; and thirdly, that strictures that are impermeable to any catheters or bougies should be treated by external urethrotomy. The larger the experience I get of the treatment of urethral strictures, is, the more convinced I am that these are the principles that should guide us in treating this disease, and that all surgical practitioners of discretion, refusing to be misled by the dictatorial assumption of some who claim for other methods unvarying success, and universal applicability, will ultimately accept them.

ART. II.—*Observations on some Forms of Enlargement of the Uterus.* By LOMBE ATTHILL, M.D., Univ. Dub.; Fellow and Examiner in Midwifery, King and Queen's College of Physicians; Obstetric Physician to the Adelaide Hospital.

THE subject of flexions of the uterus has of late years been investigated with great care, and has attracted quite as much attention as it deserves; but another condition of the uterus, intimately connected with, often indeed the cause of, these flexions, has been comparatively little noticed, I allude to enlargement of the uterus itself. I propose, in the present paper, to consider some of the forms of enlargement of the uterus which are most frequently met with in practice.

It is not surprising that the older writers should have overlooked these affections, for it is only of recent years that we have possessed the means of investigating them, and of ascertaining, with any approach to accuracy, whether, in a given case, the uterus was of its normal size and shape, or enlarged and elongated. Now, however, matters are completely altered; by means of the uterine sound we can, in the great majority of instances, decide with certainty the depth of the cavity of the uterus, and, at the same time, the bi-manual method of examining enables us to satisfy ourselves whether or not the uterine walls are thickened and hypertrophied. Some obstetric physicians still hesitate to have recourse to the uterine sound, but, after years of experience in its employment, I

am satisfied that it is not only one of the most useful, but also one of the safest instruments. In my own practice I have never yet known it productive of the most trifling injury to the patient, while the information I have obtained from its use has been of incalculable advantage. Of course, in ignorant or unskilful hands it may effect great injury; but this objection would equally hold good against the use of the catheter and many other instruments. So high is my opinion of the uterine sound that I make it a rule to introduce it, unless its use is contra-indicated by the possible existence of pregnancy, or by some equally valid cause; and I am satisfied that the day is not far distant when this will be recognized by all well-informed obstetric practitioners as the established rule.

Enlargements of the womb are met with in a very large percentage of those cases in which the symptoms are referable to the female organs of generation. Nor is this a matter of surprise when we remember the changes the uterus undergoes. In the virgin state but a couple of inches in length and an ounce or so in weight, it becomes, under the influence of pregnancy, developed into a large organ capable of containing the full-grown fœtus, and weighing several pounds; consequently any circumstance which retards or prevents the return of the uterus to its normal size after delivery, may produce, as is now well known, a condition which often results in permanent enlargement, a condition to which the term "subinvolution" is applied. But, in addition to these great changes, the result of pregnancy, the uterus every month, as each catamenial period comes round, increases in weight and, probably, somewhat in size; if, from any accident or imprudence, the natural flow is then checked, that temporary increase may become permanent, an accident which, I am satisfied, is far from being of unfrequent occurrence. Here, then, at the outset, are two palpable causes of enlargement of the uterus.

But we meet with cases of enlargement of the uterus which cannot be referred to either of these classes. Women who have been pregnant, and never have had any derangement of or departure from healthy menstruation, and women who, having conceived, have subsequently enjoyed uninterrupted good health for years, during which pregnancy undoubtedly did not take place, nor yet any derangement of menstruation occur, occasionally begin to suffer from symptoms referable to the uterus, and, on examination, these are found to be due to enlargement of that organ. In such cases I believe this condition may depend on inflammation of the substance

of the uterus, either of an acute or chronic character, on what may be termed simple hypertrophy of the muscular tissue of the uterus, on fibrous tumours developed in the walls of the uterus, and also, as all are aware, on the existence of intra-uterine tumours of any kind, whether they are polypi, fibrous, or cancerous tumours. But it is not my intention to enter at all on the subject of intra-uterine polypus, and therefore, except with reference to the question of diagnosis, I shall not allude to it further. I also purposely omitted all reference to the actual existence of pregnancy, or to the retention of any of the products of conception in the uterus, as being foreign to the subject to which I wish especially to direct attention.

To recapitulate, we meet with enlargement of the uterus as the result of—

1st. Subinvolution of the uterus after labour or abortion.

2nd. Congestion of the uterus from suppression or retardation of menstruation.

3rd. Acute inflammation of the uterus, or possibly of its peritoneal covering.

4th. Chronic inflammation of the uterus.

5th. Hypertrophy of the uterus.

6th. The stimulus given to the uterus by the development in its walls of fibrous tumours.

7th. The existence of intra-uterine tumours.

1. Subinvolution of the uterus is now a well-known cause of uterine enlargement. There is no doubt but that it is most likely to occur in those cases in which any form of inflammatory attack, whether it be peritonitis, metritis, or cellulitis, takes place subsequent to delivery. This fact has been pointed out by several writers. If, then, any patient has suffered from any such attack, the possible effect of it in retarding the normal reduction in the size of the uterus, which should take place within a few weeks subsequent to delivery, must be borne in mind, and we should, in such cases, carefully watch for any symptom indicating the presence of this condition. As a nearly invariable rule profuse menstruation is the first and most prominent symptom indicating the existence of enlargement of the uterus, depending on subinvolution, a symptom capable of being easily explained, when we bear in mind the fact, that not only is there under such circumstances an undue amount of blood contained in the enlarged uterine veins, but also that the relaxed condition of the muscular tissue of the uterus favours the

exudation of blood. This profuse menstruation does not always occur immediately; sometimes a month or two first elapses; but ere long menorrhagia shows itself, frequently of so violent a character as to assume the proportions of hæmorrhage, and, on instituting an examination, the sound reveals the true state of the case by proving that the uterus is abnormally elongated. The depth of the uterine cavity varies greatly in such cases. I have met with one instance in which it measured seven inches and upwards. But it is a mistake to suppose that subinvolution is met with only after delivery at the full period of pregnancy. It occurs also after abortion, though the possibility of this condition occurring after abortion seems to have escaped the notice of some writers.

I have already stated that the occurrence of profuse menstruation was the earliest and commonest symptom of enlargement of the uterus, the result of subinvolution. This is so generally the case that I drew the inference that the occurrence of profuse menstruation in cases of retroflexion of the uterus proved that the flexion was a secondary affection, the result of subinvolution of the uterus, and I stated this in a paper which I published in this Journal more than a year ago. I still believe that, as a general rule, this is correct, but a case has recently come under my observation which proves that there may be exceptions to this rule. A young married woman was admitted into one of our hospitals, during the past summer, for what was supposed to be an ovarian tumour. She had been confined about three months previously of her third child. Hæmorrhage had followed delivery. She also appears to have been subsequently attacked by some form of pelvic inflammation. She recovered slowly, and had not been able to nurse. The lochia ceased to appear during the attack alluded to, and menstruation had not occurred since delivery. On passing the hand over the abdomen a large, movable tumour could be easily felt through the abdominal walls; it lay to the left side, and was very painful to the touch. After a few days this woman was discharged from hospital, her case being considered unsuitable for any kind of surgical interference. As, however, she continued to suffer much distress, she presented herself as an out-patient at the Adelaide Hospital, where a careful examination, made with the aid of the uterine sound, proved the tumour to be the uterus much enlarged and elongated—in fact, it was a case of subinvolution of the uterus, with temporary suppression of menstruation. I admitted this woman into hospital, and had the satisfaction of discharging her

cured after the lapse of a few weeks, the tumour having entirely disappeared. In this case the sound penetrated to the depth of five inches.

2. The occurrence of enlargement of the uterus from any cause suddenly checking menstruation is by no means rare, but the opportunities of proving this to be the case do not frequently occur; for if an unmarried woman complains of fulness, and of pain in the head, of pain in the back, and of a sense of weight in the pelvis, and states that menstruation has been checked by exposure to cold or by some other obvious cause during the catamenial period, we are probably satisfied that uterine congestion exists, but we are not justified in making a virginal examination of any kind, unless, indeed, after a protracted trial, general treatment fails to relieve her. Again, if a married woman exhibits the same train of symptoms, the possibility of pregnancy existing precludes the use of the sound. Recently, however, I had an opportunity of verifying the fact. A widow, the mother of thirteen children, in whom menstruation had been irregular for three years, had, in June last, after a long interval, a return of the discharge. It ceased suddenly, and she suffered great discomfort from a distressing sensation of weight and bearing down in the pelvis, and of fulness and pain in the head. In her case the uterus was three inches in depth, while all the symptoms rapidly subsided under treatment. It may be objected that, in this case, we were ignorant as to what might have been the condition of the uterus previously, but here was a woman, in the enjoyment of good health, suddenly attacked, after the abrupt checking of menstruation, with distressing symptoms, and in whom the uterus was proved to be enlarged, who was relieved of those symptoms and of that condition by treatment. Is it not then fair to reason that the enlargement was a temporary condition, the result of uterine congestion, itself caused by the sudden checking of menstruation?

3. All modern writers agree that acute inflammation may produce enlargement of the uterus, and I believe that this may be the case, whether the patient suffer from peritonitis, metritis, or pelvic cellulitis. Of the two latter I have no doubt. Of enlargement of the uterus, as the result of peritonitis, I had no experience till very recently, but the following case throws some light on the subject:—

Mrs. K., aged thirty-three, was admitted into the Adelaide Hospital in May last. She was the mother of three children, the

last of whom was born in March, 1869, fourteen months previous to admission. It appears that, four weeks after her confinement, having been exposed to cold, she was attacked with severe pain over the whole abdomen. This pain after a time became localized in the left iliac fossa; and by degrees it, in a great measure, though not entirely, disappeared. At the expiration of two months from the date of this attack menstruation came on very profusely, and lasted for six weeks. She now obtained medical advice, and was treated for ulceration of the os uteri; but although the menorrhagia was in some degree checked, the pain from which she suffered again became very severe. On admission into hospital the uterus was found to be retroflected, a certain amount of granular erosion existed, and menstruation was profuse. Her greatest distress, however, arose from an incessant dragging pain which she referred chiefly to the situation of the transverse colon. The uterus was enlarged to a trifling extent. The use of a pessary and other appropriate treatment speedily improved the condition of the womb, and she returned home apparently cured. At intervals, however, she still suffered from attacks of the abdominal pain. In the beginning of October she again caught cold, and was re-admitted into hospital labouring under a well-marked attack of sub-acute peritonitis. Leeches, fomentation, and the exhibition of opium relieved her. During the course of this attack I twice measured the depth of the uterus, and found that it had increased nearly an inch in length. She did not menstruate during this attack.

This case contrasts strongly with the following one, in which menorrhagia and pain of a paroxysmal character, referable to the uterus, were the prominent symptoms. It illustrates the occurrence of enlargement of the uterus as the result of metritis:—

Early in the year I attended a lady in her confinement, in whom there existed a great tendency to *post-partum* hæmorrhage. She had a natural labour, but two or three hours elapsed before the uterus contracted firmly, and in the interval she lost a good deal of blood. She recovered slowly but perfectly, and continued to enjoy good health, till, in the beginning of September, when, having gone out boating, she caught cold. The next day she was attacked with severe pain in the region of the uterus. There was also well-marked tenderness on pressure over the pubis. This attack took place just before the occurrence of a menstrual period, but the flow, instead of being checked, appeared in increased quantity, and continued

persistently. This lady resided in a remote part of the country, and was virtually without medical aid. After the lapse of about ten days I was requested to see her. I found her in great agony, but I ascertained that this was not incessant, that she had intervals of nearly perfect freedom from pain, lasting sometimes for several hours, but then it would return with increased violence. Pressure over the uterus was always productive of distress, and increased the pain, but elsewhere the abdomen was not tender to the touch. The pulse was rapid, but not of the character which accompanies peritonitis; there was no vomiting, but a continuous though not copious hæmorrhagic discharge was present. On making a vaginal examination the uterus proved to be tender to the touch; it was evidently enlarged, and on introducing the uterine sound it passed without difficulty to the depth of five inches. I had no hesitation in pronouncing the case to be one of metritis. As already mentioned the pain was of a well-marked paroxysmal character; the tenderness on pressure over the uterus was always present, but, if the abdomen were not touched, she would have long intervals of nearly perfect freedom from suffering; then, however, it would come on and last for hours without the least intermission, a characteristic of metritic inflammation, to which Dr. West specially alludes in his valuable work on *Diseases of Women*. He state that "the tenderness of the uterus in these cases always led him to abstain from measuring its depth by means of the sound." In the case I have just narrated, however, its introduction caused no pain. The distance at which this lady resided from town precluded me seeing her again till she was able to travel, which was not for four weeks. On examining her on her arrival in Dublin I was agreeably surprised to find that the uterus, although not of its normal size, was much smaller than I could have anticipated it would be, the cavity measuring about three inches in depth.

4. Chronic inflammation of the uterus being more frequently met with than the acute form, is a more common cause of enlargement. Such cases are constantly coming under observation. They are frequently found in connexion with retroflexion of the uterus. Of course all are aware that it is a disputed point whether flexions of the uterus are a cause or a result of inflammation. For my own part, while I am far from denying that flexions of the uterus, but more especially retroflexion, may take place independent of inflammation, still I am satisfied that, in the great majority of cases,

inflammation or at least, active congestion is the primary and principal cause of these flexions, and this opinion, which I expressed in a paper published two years ago, enlarged experience has since amply confirmed. In such cases the symptoms are often very distressing. The following typical one, at present under my care in the extern department of the Adelaide Hospital, illustrates this:—

E. D., aged thirty, seven years married, has never been pregnant; about three or four years ago began to suffer from pain in the back over the pubis, and in the left groin. This pain in the groin is sometimes so severe as to prevent her being able to stand upright. Menstruation is scanty, and is occasionally suppressed for two or three months at a time. Sexual intercourse is painful. On examination I found the uterus elongated and retroflected. This case, although an aggravated one, is typical as showing the sufferings due to enlargement of the uterus, the result of chronic inflammation.

In each of the foregoing cases the enlargement evidently appears to have been due to inflammatory action attacking a uterus previously healthy, so far at least as we had opportunities of judging; but in some instances it seems to follow as the result of the treatment we have been compelled to adopt for the cure of other ailments. Eighteen months ago I was consulted by a lady for menorrhagia. She was about twenty-two years of age, and but a few months married. For a year previous to marriage menstruation had been more profuse than formerly, but not to such an extent as to attract much notice. Since marriage however she had become worse, and at the time of her consulting me, not only was the discharge very profuse, but it also generally continued to flow for more than a fortnight. A vaginal examination detected extensive granular erosion of the os and cervix uteri. This condition was in time perfectly cured, and the menorrhagia consequently ceased; but I observed that, as this condition of the cervix improved, so did the uterus enlarge, the fundus becoming heavy and globular in shape, and yet my treatment had not been characterized by the use of any severe remedy. I had hoped that, after the unhealthy condition of the cervix had been cured, and all treatment had been discontinued, that the uterus would regain its normal size and shape, but I regret to say such has not as yet, at least, been the case. I had an opportunity of seeing this lady a few days ago. The uterus, after the lapse of a year, remained unaltered; the fundus is quite globular; menstruation is scanty, and occasionally painful. She has never

conceived. My retrospect of this case is, that the treatment which it was absolutely necessary to adopt to check the hæmorrhage and cure the unhealthy condition of the uerus, excited a certain amount of inflammation of the uterine tissue, which has resulted in permanent enlargement of the organ. A similar result, I am satisfied, not unfrequently takes place from congestion and chronic inflammation, unconnected with any ulceration whatever.

5. Next I shall call attention briefly to that condition which, for lack of a better name, I have termed hypertrophy of the uterus. I mean to include under this head those cases in which the uterus slowly and unperceptibly increases in size, the cervix generally elongates, while the body and the whole of the fibrous tissue of the uterus is thickened and enlarged. In these cases menstruation often is but little altered in its character; sometimes it is slightly diminished in quantity, and not unfrequently becomes painful, but I do not remember meeting with a case in which hæmorrhage was present. I am of opinion that the condition of the menstrual functions will materially aid our diagnosis in doubtful cases; for if the enlargement be due to chronic inflammation, it will most probably be lessened in quantity; if to subinvolution or to the presence of any intra-uterine tumour, it will in general be augmented; while in cases of simple hypertrophy it is seldom altered, at least in any great degree. The pathology of this form of uterine enlargement is very obscure. We cannot in the present state of our knowledge, say why in a certain case, the cervix uteri elongates and enlarges, till by its very size and weight, it irritates and causes distress; while at the same time, in one case the body and fundus of the uterus participating in the unhealthy condition of the cervix, become heavy and elongated, and in another seem to remain in their normal condition. Excessive indulgence in sexual intercourse has been set down as a cause of enlargement and hypertrophy of the cervix, but I doubt this much. In my own practice, the case which of all others gave rise to the greatest amount of suffering and distress occurred in an unmarried woman. Miss ——, aged forty, came under my care nearly two years ago. She stated that about seven years previously she for the first time, experienced pain in the back and over the pubis, and not long after a sense of weight in the pelvis, and that her suffering had ever since gradually increased. When I saw her she could not sit up for any length of time, and

walking even a short distance caused much distress; she also complained of a troublesome itching at the vulva. On instituting a vaginal examination, the cervix was found to be elongated to the extent of at least an inch. It was thickened and hypertrophied, the supra-vaginal portion evidently participating in the abnormal condition of the organ. Menstruation continued to be performed regularly, but it was attended with much pain. She has ever since been under observation, though I have long ago abandoned any active treatment. This lady has been treated by leeching, by blistering, by the administration of the iodide of potassium, and of the bichloride of mercury, and it is hard to say which of these did the least amount of good. The uterus has steadily increased in size, evenly and universally, and with its increase so have her sufferings increased, till now she has become a complete invalid. In her case I at one time, before the body of the uterus became so manifestly enlarged, entertained the idea that amputation of the cervix might, by removing the cause of local vaginal irritation, be productive of benefit. But, as the operation certainly was not entirely free from risk, and as I became satisfied that the supra-vaginal portion of the uterus also participated in the diseased condition, I abandoned the idea. In this case I feel perfectly satisfied that the hypertrophy commenced at a very early age, and gradually increased; it bears a strong resemblance to one recorded in Vol. II. of the work on *Disease of Women*, by Bernutz and Groupel, recently published by the Sydenham Society. Another case of hypertrophy of the cervix in an unmarried woman has since come under my observation. She is a dressmaker, aged twenty-eight, an industrious woman, sitting at work for upwards of twelve hours a day. She complained of weight in the pelvis and of bearing down. She also suffered from the most obstinate constipation. Menstruation was regular, but generally accompanied by pain. On making an examination the os uteri was found to rest on the perineum; the cervix was elongated and thickened, and the fundus slightly enlarged. This woman would not come into hospital, and consequently I have had no opportunity of trying the effects of treatment, from which in truth, I would anticipate but little benefit.

Any person who has read the work just mentioned will at once see that the condition I am now referring to is very similar, if not analogous, to that termed by M. Hugier "hypertrophic allongement" of the uterus, a condition which he divides into two classes—

namely, sub-vaginal and supra-vaginal, a division the actual value of which I do not highly appreciate. I am inclined to the opinion, that although we may have enlargement of the body of the uterus without the cervix being engaged, that the cervix is never enlarged for any length of time without the supra-vaginal portion of the organ becoming implicated in the disease. I also believe that not a few of the cases recorded by M. Hugier were cases of subinvolution of the uterus following delivery, and not of the condition which I have termed hypertrophy.

But, in addition to these cases of hypertrophy with elongation of the cervix, of the body, of the uterus, or of both, we meet with cases in which there is no elongation, but the very reverse. We sometimes find the cervix shortened, drawn up, as it were, into the body of the uterus, and sometimes disappearing altogether. In such instances the body of the uterus assumes a globular form. This form of enlargement gives rise to considerable distress, and it seems specially to cause intractable irritation of the bladder. In one case, which was for years occasionally under my observation, this symptom was the prominent one, and that for which the patient sought relief.

It remains for me to allude, and I shall do so very briefly, to that form of uterine enlargement in which the organ is stimulated and increases in size from the presence of a fibrous tumour embedded in or growing from some portion of its walls. Cases are recorded in which a fibrous tumour of very small size, perhaps not larger than a nut, so stimulated the uterus that it increased to five or six times its normal size, the cavity too being proportionally elongated. These cases are most perplexing, a *post-mortem* examination alone being capable of revealing their true nature. Fortunately they are not of frequent occurrence. I have not myself met with any case in which I was satisfied of their existence. In the great majority of instances a fibrous tumour sooner or later will bulge into the cavity of the uterus, or project out on the peritoneal surface. In either case the tendency of the disease is to render menstruation more profuse; while in that form of enlargement depending on hypertrophy of the fibrous tissue of the uterus, and which is the only form liable to be confounded with the one now under consideration, menstruation, if interfered with at all, is more likely to be diminished than increased. The subject of fibrous tumours of the uterus does not come within the scope of

this communication. I wish, however, to draw attention to those cases, of by no means infrequent occurrence, where enormous fibrous growths exist in which the womb is embedded and almost lost. These cases have over and over again been mistaken for ovarian tumours, a mistake which the use of the uterine sound may help us to avoid. It tells us not only what is the length of the uterine cavity, but also whether the uterus is free or embedded in the tumour. In my opinion no operation for ovariotomy should be attempted till, by the introduction of the uterine sound, this be clearly made out.

Now, as to diagnosis. I have already stated that the sound and that alone enables us to decide as to whether the uterus be enlarged or not, but it affords us no clue as to the cause of the enlargement. A few general rules, however, if they do not enable us to give a positive diagnosis, will at least facilitate materially our decision as to the nature of any case. Thus, if we meet with an enlarged uterus in a woman, who has aborted or been delivered at the full time, even though a considerable interval has elapsed, the probability is in favour of the enlargement being dependent on subinvolution, and this opinion will be confirmed if, as is nearly always the case, menorrhagia be present. If again metritis, pelvic cellulitis, or peritonitis be present or have occurred recently, the inflammatory action is fully sufficient to account for the condition of the uterus, and it should be always borne in mind that it does not follow that the enlargement will disappear with the subsidence of the inflammation; so again in other cases, we should ascertain if menstruation has been checked or suppressed, and if symptoms referable to the uterus have followed on this, or if again, pain in the back and over the pubis was first noticed, menstruation being subsequently lessened or suppressed; in the former case we are likely to find that the enlargement depends on congestion, in the latter on chronic inflammation, while hypertrophy steals on gradually, menstruation being seldom interfered with, and it is of no small importance to decide to which cause the enlargement is due, for while much may be done to relieve the sufferings caused by enlargement of the uterus the result of chronic inflammation, treatment seems utterly powerless in alleviating those produced by simple hypertrophy of the uterus. It is, indeed, a nearly hopeless ailment, one not likely to destroy life, but to render it a burden. Then, again, if we have menorrhagia in cases of enlarged uterus, unconnected with any of the causes noticed, we may expect to meet with

ultra-uterine polypus, or fibrous tumours. It will then be our duty to clear up the doubt which exists by dilating the cervix and exploring the interior of the uterus. To effect this, I unhesitatingly recommend the plan originally suggested by Dr. Kidd ; namely, to introduce several lengths of sea-tangle bougie up to the fundus of the uterus, and to leave them there for from 12 to 24 hours. This is at once the most effectual, the simplest, the most cleanly, and certainly the safest mode of proceeding of any we possess. I generally introduce them through the ordinary Ferguson's speculum; the only difficulty consists in the introduction of the first length; if one piece be fairly passed to the full depth, others can be slipped in beside it with facility. I am disposed to think that it is better to introduce but four or five lengths on the first occasion, and the next day a larger number; the pieces previously introduced being first removed, by this means the dilatation is effected more slowly and with less risk to the patient.

With respect to the treatment of these affections, I doubt if any medicine has direct influence on that condition known as subinvolution. I have tried ergot, iron, and the bichloride of mercury in small doses without producing the least effect. Rest and local treatment alone do good. The treatment likely to result in a cure may be briefly summed up in this—first to check the menorrhagia, and next to stimulate the uterus to contract. To effect the first the vagina should be plugged on the second day of the appearance of the discharge. If the amount of blood lost be excessive, it is seldom necessary to continue this treatment for more than twenty-four or at most forty-eight hours. If, after the plug has been withdrawn, a troublesome oozing continues, I brush over the surface of the os and cervix uteri with a saturated solution of the perchloride of iron in glycerine, and after the expiration of a few days, introduce, up to the very fundus of the uterus, ten grains of the solid nitrate of silver, by means of Simpson's porte caustique; the caustic dissolving irritates the entire of the inner surface of the uterus; it causes some pain, and sometimes a little hæmorrhage follows its introduction; but if the patient be kept quiet, and due precaution be adopted, no unpleasant results are likely to follow, at least I have never experienced any in my own practice. After a time, blistering over the pubis may be practised, and tonics, especially strychnine, administered with benefit. These, of course, should not be prescribed till ample time has been given

to allow the effect of the caustic to subside. One application of the nitrate of silver is generally sufficient; and, under any circumstances, the next catamenial period should be allowed to pass before a repetition of the treatment be decided on; but should one application fail, my practice is to dilate the cervix and explore the uterus before proceeding further, for till this be done we cannot with certainty pronounce that there is something in the uterus which keeps up the hæmorrhage, and prevents it regaining its normal size. In several cases which have come under my care I have effected perfect cures by the application of the solid nitrate in the manner mentioned, and in none was the result more satisfactory than in the case of subinvolution already alluded to, which was remarkable from the total absence of menstruation.

In cases of enlargement following sudden suppression of menstruation the administration of saline purgatives, and subsequently of the bromide and iodide of potassium conjointly in full doses, will generally, if the case be recent, prove sufficient; but should it be neglected in the early stages, it will probably pass into the condition of chronic inflammation, a condition over which medicines possess little influence. The prolonged use of the bichloride of mercury, in small doses, has been recommended in these cases, but in my own practice I have not been satisfied that it produced any good effect. I have seen, I think, more benefit result from local depletion by puncturing the cervix uteri, as recommended by Dr. Hall, of Brighton, than from anything else, and I think it is a mode of treatment deserving a fair trial. To be of use this must be repeated frequently at intervals of about five days. The application, to the verge of the anus, of two or three leeches, immediately after the termination of a menstrual period, where menorrhagia is present in connexion with a relaxed and engorged uterus, also often proves beneficial. Both these methods act by relieving the congested condition of that organ, and thus facilitating its contraction. In conjunction with this treatment I recommend the administration of strychnia, with the addition of dilute nitric acid, or if the patient be anemic, with the tincture of the perchloride of iron. Strychnia is the most valuable medicine we possess in case of menorrhagia in connexion with a relaxed atonic condition of the uterus. It is, however, contra-indicated in cases of chronic inflammation, unless that condition be first relieved by local depletion.

In cases where the uterus has become enlarged and hardened, as the result of chronic inflammation, the use of the waters of

Kreuznach seems to have a very beneficial effect, and if the patient's means are such as to admit of her visiting that place, a trial should be made. As to hypertrophy of the uterus, I know of no treatment likely to effect good.

In cases of enlargement of the uterus from inflammation of an acute character, I believe rest, the exhibition of opium, and warm poultices over the abdomen to be the means on which we should rely; depletion, if practised at all, should be in a limited degree by the application of a few leeches. Mercury I consider to be not only useless but actually deliterious.

ART. III.—*On some Requirements in Clinical Teaching in Dublin.*—By WILLIAM STOKES, M.D., D.C.L.; Regius Professor of Physic in the University of Dublin.[a]

As among the members of this Society are many whose labours in Clinical Medicine have largely advanced the Irish school, I trust that the following observations will be received with indulgence, and not attributed to any spirit of carping or fault-finding.

It is admitted on all hands that during the last half century a great school of Practical Medicine, Surgery, and Midwifery, has in Dublin grown to such dimensions, and established such a character, as to constitute at least one source of legitimate national pride, so much wanting in Ireland; a possession which to any country is more to be valued than wealth or power, or the barbarous triumphs of war.

But to all thinking men who have watched the rise, progress, and actual state of the Irish School of Medicine, the question presents itself—Are we, so far as modern methods of research are concerned, keeping up with the progress of medical science? are we standing still and drawing on our acquired prestige? or if advancing, are we not following merely the beaten path, irrespective of the growth of knowledge in other places? It must be confessed, looking at the body of literature relating to scientific medicine, that our share of it is comparatively small.

The term Scientific Medicine is one of late date, and is applied

[a] Read before the Medical Society of the King and Queen's College of Physicians, Nov. 16, 1870.

to that body of facts, or supposed facts, which have resulted from the employment of methods based on the use of physical investigation for the elucidation of health and disease, by which alone can be obtained or promoted the quality of exactitude in medical knowledge. But it does not, and ought not, to convey the idea that our predecessors were not scientific, or that their results, as tested by their application to the healing art, are to be contemned. In truth, the labours of the so-called scientific schools cannot be held, at least as yet, to be worthy of occupying a higher platform than those of their predecessors, so far as the treatment of disease is concerned; and it may be a question whether, during times when physical diagnosis and research were but little resorted to, a greater power, a larger grasp of mind, did not distinguish the physicians of past times from many of those of the present period.

It must be remembered that medicine advances in two directions; first, in the observation of those manifest phenomena of disease, whether or not influenced by therapeutic measures, which lie on the surface, and are, as it were, plain to the naked eye; and next of those which lie deeper, and whose existence, nature, and relations to the manifest, require for their elucidation the use of methods based on physical knowledge, by which alone scientific accuracy can be approached or attained.

It is in the latter direction that we are deficient. It seems to be quite natural, even perhaps to be desired, that certain schools should excel, or have a greater fame in some directions than others. But in a great school should any of the branches of applied science, —any mode of discovering truth—be conspicuous, by what may be virtually called their absence?

The modern or scientific method may be considered with reference to normal anatomy and physiology on the one hand, and to pathology, including morbid anatomy and therapeutics on the other. It is mainly in reference to the latter subjects that it bears on practical medicine. Now, as it is in this direction that the Irish school has advanced, we may consider the subject as in relation to the improvement of the healing art.

It is here that we are behind hand. The studies of structural anatomy, of pure physiology, and of organic chemistry, all-important as they are, have not as yet given us any great addition to our means of treating disease. But yet, although in these departments so little has been effected by the Irish school, that circumstance

should not detract from its high character in reference to practical medicine. What is wanting is a larger application of modern methods to the study of pathology, morbid anatomy, and therapeutics.

Some will say that by our system we have made many good physicians and good surgeons. Granted; yet is not this an inducement to seek to produce a still higher class, remembering that the phenomena of disease are to be studied like those of all other natural objects? If we wish to give our teaching the character of scientific accuracy, we must not neglect the methods which promote that end, and as we have attained a good position in practical medicine and surgery, so we should see that no means by which that eminence may be preserved or advanced is neglected.

The studies of pathology and of morbid anatomy in Dublin, have advanced in three directions—namely, in the observation of conditions which lie on the surface and are plain to the naked eye. And again, and with greater value and singular success, in the relation of these conditions during life to the symptoms and signs of disease. And lastly, in the record of the combinations of local changes. Of the importance of our results I need only point to the Transactions of the Pathological Society. That body has now been in existence for more than thirty years, and is steadily increasing in its numbers and utility; yet it must be admitted that in microscopic morbid anatomy, and in pathological chemistry, the results of its labours are comparatively small. Why is this? Why is the study of morbid anatomy and pathology in Dublin wanting in the use of methods calculated to ensure scientific accuracy? May it be that we have not begun from the bottom? Have we afforded our students the primary instruction or even the means of teaching themselves, or shown them that knowledge, which implies power, is advancing all around us and them, while we or they are standing still or advancing in but one groove? Or have they been shown the excellence of labouring for truth for its own sake, irrespective of its immediate consequences?

It will be superfluous to enlarge on the dimensions to which the various branches of scientific medicine have attained, and impossible to predicate to what limits they may yet extend. All its instruments are adapted to the study of the higher anatomy and physiology, as well as to that of disease, and the action of remedies. All have the one end—accuracy—each of them a means the more for the advance of the healing art. We may put aside the ophthal-

moscope, the laryngoscope, the stethoscope, and the endoscope, which last, thanks to the labours of Dr. Cruise, Dublin may almost claim for her own, and pass to the microscope, the spectroscope, and chemical research.

It will be unnecessary for me to dwell on the importance, the indispensableness of the latter methods of research to the solving of questions in physiology and morbid anatomy, especially in their relations to practical medicine. In truth, morbid anatomy, so far at least as changes of structure revealed by the unassisted eye, are concerned, seems almost to have reached its limits, and it must always be remembered that the changes thus observed are in most instances but advanced steps in the procession of diseased actions. How constantly are organs noted as being free from disease, in which a microscopic examination would have detected a great departure from health. Let those who are content that we should, in our schools and hospitals, go on in the old way, compare what has been done among us in microscopic morbid anatomy, with the labours of Ormerod, Bennett, and other British observers—to say nothing of Virchow, or of Cohnheim—in the direction of Physical investigation, as applied to practical medicine, and they will find it difficult to deny that great deficiencies exist in our system.

The study of medicine in its modern, or more scientific aspect, may be considered in reference to normal anatomy and physiology on the one hand, and to pathology, including morbid anatomy and therapeutics on the other. It is mainly in the latter direction that it bears on practical medicine. As the advance of Irish medicine has been almost exclusively in what has been called the practical direction, we may confine ourselves at present to the value of the scientific method in relation to the healing art. It is the deficiency in our schools and hospitals, as to the use of the scientific method that demands to be pointed out. In a few of our hospitals practical instruction is given by the medical officers to such students as choose to avail themselves of it, but we want a larger application of the modern methods to the studies of disease at the bedside, of morbid anatomy, and of therapeutics.

With respect to the Continental schools, especially those of Germany, there can be no doubt that in them a far greater number of labourers in scientific medicine exist than in Great Britain. This is in part owing to the difference in the institutions, and social conditions of the countries; and a large portion of the Continental

work is devoted to the study of normal structure and function, which has not as yet been productive of any great addition to our means of treating disease. Yet, every day is altering the relative numbers of workers at home and abroad, and it is at all events certain that while British investigators lean to the application of scientific research in a practical direction, they exhibit more of the modesty of true science than do many of our Teutonic brethren. They do not affect to occupy a higher platform than the investigators of other countries. They may be, as a late writer somewhat sneeringly remarks, in the class of the *autodidacten.* But are they the worse for that? May they not be the better?[a]

The addition of the spectroscope to our means of research promises results, the importance of which are incalculable, and it is not improbable that by its assistance many questions relating to essential, as distinguished from local disease, may yet be elucidated, especially when its results are taken in connexion with those of organic chemistry. Its marvellous powers of qualitative analysis are shown by the researches of Mr. Huggins, and Dr. Millar, into the chemical composition, even of the fixed stars, while its powers, as regards the composition of the colouring matter of blood are established by the researches of Professor Stokes of Cambridge, and of Hoppe. The spectral phenomena of blood, as shown by Dr. Letheby and Mr. Sorby, reveal its presence, whether the stain be recent, or of many years' duration. It is unnecessary to allude to the researches of

[a] In truth, the tone of superiority adopted by some German authors is simply offensive, not to British investigators only, but to those in other portions of the world In reference to this matter a writer in the Archives of Medicine, No. xvii., observes :—

"It has been truly said that Science is of no nationality, that a man who interrogates nature successfully, whether he works in Germany, or Siberia, or Mexico, in London, or Boston, at once becomes a Member of that great and ever-extending Republic, so pure that wrong is righted ere it merits its name; so perfect that every change, mighty or insignificant, is welcomed and adopted without producing the slightest strain. But such dreams must be dispelled. Germany has spoken. The investigation of nature's minutest and most delicate secrets, is her prerogative only. A new Republic is inaugurated; authority exacts submission, commands obedience. Masters must have docile pupils, and the pupils must accept, not doubt; obey, and not inquire; must worship and not dispute." . . . .

"A German author, whose work has just been translated into English, acknowledges the compliment paid by us to German microscopic work, thus gracefully:—'In the region of scientific medicine, the Germans enjoy at the present time an undisputed pre-eminence. Their medical books have taken possession of the markets of the world, and their larger schools are themselves like markets, in which representatives of all countries appear, in order to exchange gold for the higher culture.—*Professor Stricker, Nature—Sept.* 1, 1870.'"

Dr. Bence Jones, and other English observers, but it appears more than probable that before it can be affirmed that the nature of all essential diseases is as fully investigated as our powers will admit of, the blood in these affections, and in different stages of these affections must be submitted by competent observers to a searching microscopic and spectroscopic examination.

The deficiencies in our school as to scientific medicine may be considered in two points of view.

First, the small amount of original work it has produced. We have, it is true, a few labourers in this direction. Their names will occur to every one, and they deserve all honour, not for the high value of their labours only, but for their pursuing their inquiries, based on physical science, in various directions, without example and without assistance.

Secondly, we must look to them in relation to the teaching of the crowd of students which annually frequents the schools and hospitals of Dublin. Here, indeed, we have but little to show. It has been attempted to form microscopic classes in the University and in the Royal College of Surgeons, but the success of these efforts has been of a shadowy nature, and for an obvious reason—namely, that one great element of interest to the student must necessarily be withheld if we look to the practical application of scientific medicine. Instruction of this kind, which is confined to a school, has no necessary connexion with hospital study, and it is deprived of its demonstrative character in relation to disease and its results, watched in the wards, and investigated in the dissecting room of the hospital. Such a mode of instruction is deficient in value as to its application, and so must want interest.

Now, putting aside the Palatial institutions which are on the continent in connexion with the great hospitals, for the scientific study of clinical medicine, let us look nearer home and we must admit that, compared with London and Edinburgh, Dublin is deficient not alone in the investigation of disease by the modern application of physical science, but also in affording means of instruction to the students in this direction. The names of Queckett, of Beale, of Bowman, and of Carpenter, recall the labours of the London school. At Edinburgh, Professor Bennett has nobly shown how medical observation and progress are promoted by physical research, and he has established a physiological laboratory, which is attended by a large class of students. In that stately and beautiful structure, the New Museum at Oxford, exist the most complete

arrangements, available to every student for all studies based on physical science in physiology and pathology, while at Cambridge similar arrangements have been completed on a liberal scale.

It is clear that if we desire to keep up with the progress of science in its application to medicine, we must base the effort on the teaching of our students, and trust to time for the development of men, who though at first but few out of many, will ultimately stand out in relief and become themselves teachers and discoverers. We must henceforward provide instructions in all the methods by which physical science is brought to bear on the advance of medicine. Our school teachers and our hospital officers, and especially the latter, must see to this, for they have the opportunity of showing how the application of physical methods elucidates the phenomena of living disease, and so subserves the art of healing. Hospitals should not be considered merely as means of teaching elementary medicine and surgery. In them are to be met infinite complications relating to disease, the saving of human life, the alleviation of human suffering, and the whole subject of therapeutic science. We are bound to show how physical science every day enlarges our means and subserves those ends.

The medical officer of a clinical hospital should not be content with merely permitting his students to follow his teaching and his practice in the daily work of the wards. It is true that by observation and self-instruction they in many cases derive great advantage in becoming qualified for the ordinary routine of practice. But more is necessary, and a larger view of the relations between the teacher and the student seems requisite. Hospitals should be schools for the general enlargement of the mind, schools of applied ethics and applied science, and it becomes our bounden duty to train the students' minds by the use of all scientific methods of observation to the practice of exactitude and the habit of definite thought. The work must not be done grudgingly, and we may hope to see established in every hospital in Dublin a Physical laboratory furnished with such apparatus and appliances as the science of the present day requires for the investigation of disease. Even if nothing more than this were done, and the class were supplied with the means of instructing themselves, a great reform would be effected, promoting the advantage of the patients and the students, and also the future character of the Irish school. But more is necessary, and we may trust will ere long be accomplished.

In these countries, owing to various circumstances, we are not yet in a position to follow the example set us in the great schools of Vienna, Berlin, Breslau, and even Stockholm, where arrangements exist, the completeness of which may be taken as a measure of how far those places are a-head of us, in the scientific study of medicine. To the great hospitals are attached spacious and richly provided buildings, with theatres for instruction, and laboratories for practical work, in which every pathological specimen is submitted to the most advanced physical examination, optical and chemical, by eminent pathologists. Records are kept of every microscopic and chemical result. The hospital class is instructed by the professor, while assistants train the students to practical work in the laboratory, or show them the application of the physical methods to the cases in the wards. Thus they are taught how to observe, how to connect the physical with the vital phenomena, how to think, and how to discover, and all this outside the pursuits of the higher anatomy and physiology, for which the hospital is less fitted than the school.

In our first attempts at making such arrangements in the Dublin hospitals, no great amount of expenditure will be necessary. A commodious and well-lighted room within the walls of the hospital is requisite. This should be furnished with a few microscopes of moderate power, one of which may be fitted with a spectroscope; a limited chemical apparatus for qualitative analysis, with fitting re-agents, a washing stand and arrangements for the safe-keeping of instruments, which in time would include the ophthalmoscope, laryngoscope, and endoscope, are all that in the first instance would be demanded. A few standard works, such as those of Beale, Bennett, Carpenter, and Parkes, might form a library of reference, which should be kept in the laboratory, so as to be always at hand.

Already, in consequence of a representation of the medical officers to our enlightened Board of Governors, a physical laboratory is in course of preparation at the Meath Hospital. Mr. Spencer, of Grafton-street, will furnish the instruments, and advise upon the general arrangements. Putting all other considerations aside, it may be anticipated that by the facilities which the medical officers will thus have in the diagnosis and treatment of disease, the sick poor in hospital will be greatly benefited. In private practice the service of the expert can be always, at least in cities, commanded; but as regards the poor the case is widely different, and it may be, that under the proposed

arrangement, by which it will be possible even in the one case to have repeated physical investigations carried out, the position of the patient in hospital may be better than in private practice.

But it is necessary that something more should be done than the making provision for the self-instruction of the students, and this opens up some considerations, the importance of which is very great. There can be little doubt, so far as our University students are concerned, that a certain portion of them would largely profit by the physical laboratory established in the hospital, and by the library of scientific research. But it is to be feared that many of the class would not have had that previous instruction in physics which would enable them to understand the nature of the necessary instruments, the value of the phenomena displayed, the mode of reasoning on them, nor even the terms employed. Such is the result of the so-called preliminary educational system, which in general may be taken to imply an education preliminary to the stoppage of all general training of the mind.

If the establishment of the Physical laboratory in each hospital of instruction be carried out, it will become necessary, for more reasons than one, that an officer or officers should be appointed, whose duty would be to take charge of the laboratory and its apparatus, to train the student in the use of instruments, to assist them in making observations, while, by free access to the cases, he would be able to give to his teaching the value and interest of demonstration in regard to clinical medicine. It is not to be expected that the senior physicians and surgeons of an hospital could be so conversant with the modern modes of physical inquiry, as to be able to train the students in these directions. Their function is to teach them how to reason on the phenomena thus observed, and as the officer in question must act as referee or consultant to the physician or surgeon, in every case requiring advanced physical investigation, he must have passed the pupillary condition, and have become a member of the profession. Such an officer should have full opportunity of carrying on and even of publishing whatever original investigations he might choose to follow, for by this alone can he be elevated from the position of a servant to the rank of an independent scientific man.

That arrangements of this kind would greatly enlarge the scope of our hospitals, as places of professional study, and eminently advance the character of the Irish schools, must be obvious to all. That they will sooner or later be effected, is tolerably certain.

Such appointments might be made to be held for a definite time, say from three to five years, and then every hospital of instruction in Dublin would be able to furnish the profession, not merely with the ordinary class of medical students, whose admission into the hospital implies the payment of a fee, and who must be incompetent to benefit the pupils in the way of instruction, the physician in the way of observation, and medical science, in the way of original research, but with young medical men experienced in disease, and experts in the scientific study of medicine.

In many of the London hospitals officers of this kind are now appointed under different designations; in one institution in Dublin, the Mater Misericordiæ Hospital, appointments of this nature have been lately created; while in Germany the office of *Privatdocent* corresponds to that which has been indicated.

If we consider the life of a professional man who has to live by his practice, it must be admitted that the opportunities being given, the most fruitful period for the augmentation of that knowledge, which will make him eminent as an observer or a discoverer, is the time while he is still young, emancipated from the toils of a coercive education, and before he becomes burdened with the cares of his profession.

By adopting the course which has been indicated, it is evident that the utility of our hospitals would be greatly enlarged, as it would extend to a much higher class of students than those who at present frequent our wards. These latter too would receive a greatly enlarged education, and the result to the country and to science would prove of the highest value. And it is to be remembered that the main consideration which induced the House of Commons to continue the hospital grants for Dublin, was their importance to the country in an educational point of view.

There are at least ten independent hospitals of instruction in Dublin, each with its own traditions, customs, and methods of study. To this circumstance much of the success of the Irish school is attributable. If each of them were able to send out every three or five years, one or two of such highly trained men, what an advantage it would be, and what an impulse would it give to the cause of scientific research in our country; and what a great advantage it would be to the sick poor in hospital, if the physician had at his hand the means and the power of elucidating questions which require the application of all or any of the means of physical

research. It is not to be held that the microscope or chemical analysis are not in use in our hospitals; but the system is imperfect and unsatisfactory, as regards the general character of the school.

In thus seeking to enlarge the scope of our hospitals as places of medical study and advancement for those already qualified to enter the profession, certain difficulties will have to be encountered. But progress controlled by judgment will in the end prevail. If from circumstances easily to be appreciated, the officers of an hospital, many of them no longer young men, all of them engaged in, or looking for practice, cannot afford time for the study or teaching of those branches of the subject, which are necessary in the present advanced state of pathological science, why should not full provision be made for assisting the hospital officer in the necessary instruction of the student? and why should our hospitals remain as sealed books to so many who are ready and willing to avail themselves of the opportunities therein afforded of original investigation?

An objection to arrangements of the kind has been raised, which is somewhat singular. It is that young men who may have served in these offices, might be held to have thereby established what is termed a claim to be elected into a higher office at some future occasion. Granted it would be so, and what then? Will it not be of the greatest advantage to any board of electors that among the candidates for the honour there should be individuals whose labours have added to science, and whose success in teaching has advanced the school and reflected honour on themselves. And surely the greater number of such candidates as may present themselves, the better for the hospital, for the country, and for the reputation of the school.

But as regards the whole subject there remain one or two considerations to be dealt with. For some years past the mind of England has been largely turned to the importance of Preventive, as distinguished from Curative medicine. In 1867, the British Medical Association held a memorable meeting in the University of Dublin. On the report of the Public Health Committee being brought up, it was advocated that the Association should apply to Government to appoint a Royal Commission to inquire into the state of the sanitary law throughout the kingdom, with a view to legislation. The commission was appointed. It was subsequently recast, and it was thought advisable that its

inquiries should at first be confined to the state of the sanitary law in England. Doubtless a similar course will be taken as regards the remaining divisions of the kingdom. That commission under the chairmanship of Sir Charles Adderley, M.P., has laboured with signal energy, and its report will be ready for the approaching session of Parliament. The confused mass of sanitary laws will be consolidated. Enactments may be hoped for calculated to preserve and improve the health and well-being of the community at large, and we may look for laws which will constitute some of the most enlightened enactments of the Government. For all those who will be employed under this law, the necessity of the use of physical methods is obvious. If the medical profession desires to keep in its hand the administration of the great interests and the practical application of Preventive Medicine, it behoves us to see that our students are prepared to take office under a system which will involve a large application of physical knowledge; for reports on microscopical, chemical, and meteorological questions, will be constantly required. This commission, the first fruits of the long and enlightened advocacy of Dr. Rumsey in favour of a large and comprehensive system of state medicine, is not likely to adopt the extravagant theories of some sanitarians, in respect to the etiology of diseases or our power of preventing epidemics. Its members will seek to bring before the State the importance of promoting the health and well-being of the community at large, by providing them with pure air, pure water, unadulterated food, and wholesome dwellings. These objects involve the questions of sickness, mortality, nuisances, over-crowding, drainage, habits, food, water supply, ventilation, epidemics, vaccination, contagion, endemical disease, and occupation. By regulating the laws relating to these matters, and by bringing them up to the state of present knowledge, it is hoped confidently, that an improved condition of public health will be produced, by which the population will be better able to resist epidemical or endemical disease, or when it does come to bear it better.

In contemplation of this the University of Dublin has instituted a diploma in state medicine of a high class; the candidate for which must not only be fully qualified in arts and medicine, but will be liable to be examined in pathology, and in vital and sanitary statistics, engineering, chemistry, natural philosophy, meteorology, and forensic medicine.

To conclude, I have endeavoured to call the attention of those who

are interested in the prosperity and future usefulness of the Irish School of Medicine, to the fact that in Dublin we have kept pace with other schools in but one direction, while in experimental physiology, in organic chemistry, and in the use of physical methods in advancing practical medicine, we are so far behind hand. I believe that this is owing to neglect of practical instruction in relation to hospital study, or at least to the want of those means by which the student might, to some degree, educate himself. We have not been reading the signs of the times, and it is plain, at all events, that these matters should be seriously considered by us all.

Let me not be understood, however, as seeking to depreciate that body of practical medicine which—advancing since Hippocrates—has grown to such dimensions, even before the times of Avenbrugger and Laënnec. What is wanting is the wider admission of the great truth, that—in the development of the highest attribute of the physician, the power almost instinctive in diagnosis, prognosis, and treatment—the various physical methods give a priceless means, the more of arriving at and securing that scientific accuracy, the value of which to the well-being of man is simply inestimable.

Since the reading of this paper I have received a communication from Professor Stokes, of Cambridge, some extracts from which will be read with interest, as referring to the question as to how far the spectroscopic examination of blood might be expected to throw light on essential as distinguished from local disease :—

"Cambridge, *December 27, 1870.*

"Purple cruorine is an active absorber of oxygen, while the scarlet is, for an organic substance, an active oxydizing agent. Naturally it does not come up to such powerful inorganic oxydizers as peroxide of hydrogen or permanganate of potassium. Thus urea, which is easily oxydized by permanganate of potassium, does not get oxydized in the circulation, but needs to be eliminated by the kidneys.

"3. It is not to be expected that in diseases more than a very small portion of the cruorine should be altered. Any extensive alteration could hardly, it seems pretty certain, be compatible with life. Consequently, if the spectrum of diseased blood be examined, the observer must expect to encounter *in the main* normal cruorine. This acts so powerfully and so distinctively on light that the chief features of the spectrum will be those of normal cruorine. But as this substance is pretty transparent for some parts of the spectrum—transparent, for example, to a very considerable degree for the greater part of the red—it is possible that febrile blood might show something which would be indicated by a new

band in this part of the spectrum. In short, the observer is not to expect to find the spectrum of normal cruorine absent or changed, but, over and above the bands due to normal cruorine, he *may* find a band or bands in the part of the spectrum which it leaves unattacked.

"One such band in the red, not indeed seen directly, but produced by the action of a re-agent, seems to me very likely to occur. I have often thought of taking the matter up, but never have done so. Will you do so, as you have the opportunities afforded by large hospitals?

"If hydrosulphate of ammonia be added to a dilute solution of normal blood, with or without the previous addition of an alkaline carbonate, no immediate effect is produced, but in a few minutes at the outside the scarlet cruorine (or rather hæmoglobin) is reduced to the purple or deoxydized.

"But if gallic acid, with an alkaline carbonate, be first added to the blood solution, and then after a little while hydrosulphate of ammonia, instantly a very distinct band is developed in the red not attributable to either hæmoglobin (cruorine) or to hæmatin, either oxydized or reduced. There are other substances besides gallic acid which, when used in a similar way, cause the appearance of this band. If a solution of normal blood be left standing till it has begun to putrify, the addition of hydrosulphate of ammonia at once develops a similar band. It would be interesting to observe whether freshly drawn febrile blood would agree with putrifying blood in this respect. The observer can familiarize himself with the band by adding gallic acid to a solution of normal blood.

"I have obtained some evidence that, whereas in its action on the substances introduced into the blood from the food, scarlet hæmoglobin imparts to them its semi-free oxygen, oxydizing them while it is itself reduced, and is then re-oxydized in passing through the lungs, in the case of gallic acid, on the other hand, the oxydized hæmoglobin forms a permanent compound with the gallic acid—a compound which does not split up into reduced hæmoglobin on the one hand, and products of oxydation of gallic acid on the other. The appetency of gallic acid in an alkaline solution for oxygen seems satisfied by its entering into a combination with the oxydized hæmoglobin.

"I should have said that we have optical evidence of the formation and existence of this compound in the removal of the absorption bands of hæmoglobin, whether of the oxydized or reduced, when the gallic acid is used in excess; but the new spectrum shows nothing distinctive till hydrosulphate of ammonia is added. With a smaller quantity of gallic acid a portion of the hæmoglobin would remain free, and show the behaviour of normal hæmoglobin, while another portion would be engaged, and would show the new band on the addition of hydrosulphate of ammonia.

"When blood putrifies, I suspect that some of the products of putre-

faction behave like gallic acid, entering into combination with oxydized hæmoglobin, instead of reducing it and being themselves oxydized by it. And the same may, perhaps, be the case in instances of low fever, even in the circulation. If so, the hæmoglobin so engaged would be clogged, as it were (the word is to be understood in a chemical not a mechanical sense), and rendered incapable of discharging its normal functions. It would be an interesting point to determine ; and if evidence were obtained of the existence of such a compound, the investigation might even throw light on the theory of remedies.

"From what you tell me of extreme cases, I think it very likely that in such cases, at any rate, the addition of hydrosulphate of ammonia, and immediate spectroscopic examination, would reveal an abnormal condition of the blood. The blackness in the form of fever which you have lately had in Ireland, I should suppose to indicate insufficient oxydation, perhaps from a portion of the blood being in a putrid state, and so making abnormal demands upon the supply. The detection of insufficient oxydation would demand special precautions in the manipulation, so as to avoid access of oxygen by exposure to the air. I should have more expectation of useful results from the hydrosulphate of ammonia test.

"Hæmoglobin is a very unstable substance, at least very easily decomposed by certain re-agents, acids especially. It is difficult to dry it without more or less decomposing it. With certain preventions I believe it may be dried, and when once dried may be kept in a dry state without decomposition. I should question whether such circumstances are likely to occur in a casual blood stain.

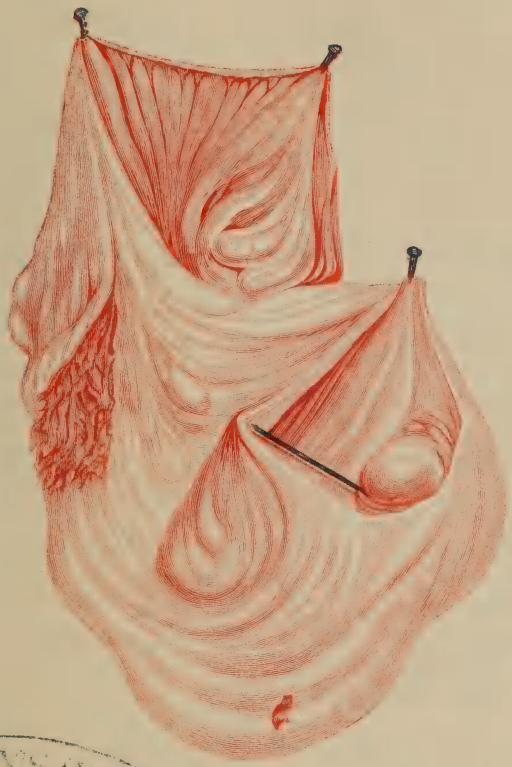
"But hæmatin, which is the coloured product of decomposition, is an extremely stable substance, and it yields spectra hardly less distinctive and delicate as tests, than those of hæmoglobin itself.

"I have called the substance hæmoglobin rather than cruorine, because I think Hoppe-Seyler has a right to name it, and he has called it hæmoglobin.—Truly yours,

"G. G. STOKES."

ART. IV.—*Contributions to the Surgery of the Genito-Urinary Organs.* By JOHN K. BARTON, M.D., F.R.C.S.I.; Surgeon to the Adelaide Hospital ; Lecturer on Surgery, Ledwich School of Medicine, &c.

THE morbid conditions to which the genito-urinary organs are subject form a group of diseases, important alike from their great extent, their constant occurrence, and from their frequently demanding for their successful treatment a high degree of operative ability on the part of the surgeon.



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Dr. Barton's Case of Ovarian Disease.

With the view of making some contribution towards the practical surgery of this important group, I now purpose relating a few selected cases, with brief observations upon the special points of interest in each.

The cases are as follows:—

1st. Calculus in female bladder, formed round a hair-pin—Removal—Recovery.

2nd. Large urinary fistula, with loss of substance in penile portion of the urethra—Closed by two operations.

3rd. Urinary fistula through scrotum—Closure without operation.

4th. Imperforate-hymen—Retained menses—Operation—Recovery.

5th. Multilocular ovarian tumour—Ovariotomy—Fatal result.

Case I.—*Hair-pin in Female Bladder, forming Nucleus of Stone—Removal of Foreign Body and Calcarious Deposit—Recovery.*—B. D., a married woman, twenty-eight years of age, residing in the county Meath, was admitted into the Adelaide Hospital, under my care upon the 25th of November, 1869. Upon examination the symptoms were all found to point to the existence of a calculus in the bladder. The urine was voided very frequently, with great pain and straining; the abdomen was painful on pressure, particularly over the region of the bladder, while, after voiding the urine, severe pain was felt along the sciatic nerves, as well as in the perinæum and sacrum. The urine contained a quantity of ropy mucous blood and pus—S.P. 1020. The patient could not stand without difficulty, on account of the pain down the legs, even sitting up in bed causing an increase of pain in back, &c. At night she complained that she could not sleep on account of a peculiarly distressing, burning sensation felt in both feet.

The history of the case was the following:—

In the first week of June ('69) the patient had inserted an ordinary hair-pin into her urethra, the rounded or bent end being foremost. This passed in so readily as to get beyond her control, and finally it slipped into the bladder altogether. At first she felt no inconvenience, nor indeed for three months did she feel any urgent symptoms. In September, however, she began to be troubled with frequent micturition, and gradually all the symptoms already detailed became established, and increased to such a degree as to compel her to seek for relief. She had, however, continued to act as a thorough-servant up to a fortnight before her admission to

hospital. She had two children, and was at this time seven months advanced in pregnancy.

A sound introduced into the bladder at once struck a foreign body, which felt perfectly fixed. Neither its shape or size could be accurately ascertained, on account of the irritability of the bladder, and the way in which it was contracted upon the stone; but this was of little consequence, as there could be no doubt that the foreign body felt was calcarious matter deposited upon and around the hair-pin as a nucleus. The first step in the plan I adopted for the removal of this hair-pin stone was that of dilating the urethra by means of sea-tangle. Upon the 29th of November the dilatation was commenced—a medium-sized gum-catheter was first introduced, and the bladder washed with warm water; a narrow piece of sea-tangle bougie was then gradually introduced upon each side of the catheter, to which they were secured, and then the catheter was carefully tied on. All being thus secured, a half-grain morphia suppository was placed in the vagina. Next day the patient said she had suffered all night, but was now easy. Upon examination the sea-tangle, which had kept in its place perfectly, was found swollen, and about three times its size when introduced. When the catheter and all were withdrawn, the urethra was found sufficiently dilated to admit the tip of the finger into its orifice, but the upper part was not dilated to the same extent; the catheter and three new pieces of sea-tangle were then introduced and tied in, and a half-grain morphia suppository was placed in the vagina. Upon the following morning, the 1st of December, forty-eight hours since the dilatation had been begun, it was found to have been carried sufficiently far so as to admit the introduction of the forefinger into the bladder. The patient was consequently placed under the influence of chloroform, and secured in the position for lithotomy. The forefinger of the left hand was then introduced through the dilated urethra into the bladder, and immediately a catheter was slipped in by its side, through this three or four ounces of warm water were injected. The position and size of the body to be removed could now be accurately determined by means of the finger in the urethra, and the index finger of the right hand in the vagina. It lay obliquely across the floor of the bladder, apparently about three to four inches in length; rough on the surface, and about as thick as the middle finger; it could not be moved in the bladder, but when moved between the two fingers, the bladder moved with it. The catheter having been withdrawn, a long narrow urethra forceps was

introduced and readily made to grasp the calculus, which, however, at once broke and came out piecemeal; the hair-pin, with about a third of the concretion still adhering to it, was, after considerable difficulty, finally removed; the difficulty arose from the way in which it was fixed, and it was at last safely removed by gradually working it round, until its extremity was brought opposite to the orifice of the urethra, when it was readily extracted; all loose pieces of detritus were removed with the forceps, and then the bladder was thoroughly washed out with warm water. After this the patient was removed to bed; a soft plug of lint wetted in carbolic oil was introduced into the upper part of the vagina to support the bladder, and thus facilitate the discharge of any remaining detritus. To the vulva, cloths wetted in iced water were constantly kept applied, and a draught containing thirty-five minims of the solution of muriate of morphia was ordered to be administered, if necessary, at night. The hair-pin, with its surrounding concretion, weighed eighty-four grains—this, when chemically examined, was found to consist of lithic acid, phosphate of lime, and ammoniac-magnesian phosphate.

Upon the day following the operation the patient was found to have passed a quiet night, no opiate being required. Pulse, 90; tongue clean; bladder was injected with six ounces of warm water, each ounce containing six drops of the acidi. nit. dil.; the plug was removed.

December 3.—Patient appears to have a severe cold, but the symptoms, as far as regards the bladder, are improved, as the urine is now kept for about half an hour; the bladder again injected.

December 4.—The urine is still slightly alkaline, and a few blood discs are found, when examined by the microscope, but no phosphatic crystals.

When this patient left the hospital, which she did in about three weeks after the operation, all symptoms of urinary irritation had ceased, and she was able to retain her urine for about two hours. I have not been able to learn anything of her since.

Various foreign bodies have been found forming the nuclei of vesical calculi. Sometimes a congenital error of development has supplied such a nucleus, as in a case related by Sir B. Bodie, in which he removed a calculus from the bladder of a young lady, which was found to contain "a small portion of bone of an irregular shape, and two imperfectly formed human teeth." More frequently, however, such foreign bodies have been introduced

from without. The same author relates the case of a gardener who endeavoured to dilate a stricture of the urethra with which he was affected by introducing a flower-stalk as a bougie—the extremity of which broke off, and lodged in the bladder. He subsequently underwent lithotomy, when an oblong calculus, containing the piece of flower-stalk in its centre, was extracted. My colleague, Mr. Richardson, exhibited a calculus before the Pathological Society in 1856, which contained a leather thong, from 5 to 6 inches in length, coiled up, thus forming the nucleus of a large stone, which was extracted by lithotomy, in America. The female bladder has, however, been much more frequently found to contain foreign bodies than the male; in fact the list of miscellaneous articles which have found their way into this cavity is surprising. Thus, Sir B. Brodie had in his possession a stone removed from the female bladder, which upon being sawn across was found to contain a hazel nut in its centre. The late Sir. P. Crampton removed a needle-case from the same receptacle, and other surgeons have found tooth-picks, hair-pins, peas, beans, pebbles, &c., &c. The kind of calculus which forms in all such cases seems to be invariably the same, viz., the triple-phosphate and phosphate of lime, or fusible calculus; this always breaks down in the jaws of the forceps, and thus there must be special care taken in the removal of detritus by frequent washing of the bladder. The plan adopted for the dilatation of the urethra in the case just related, answered the purpose well; it was not excessively painful, sleep being attained by a morphia suppository during the two nights whilst the dilatation was proceeding; nor was it tedious, and the result as regards the subsequent powers of the bladder to retain the urine was in this case encouraging, but the patient escaped from observation too soon to admit of any positive conclusion upon this important point.

The subject of urinary fistulæ is one of much interest to the surgeon. The difficulties which surround the management of such cases, and which frequently prevent the most skilfully performed operation from succeeding, demand the most careful consideration from the operator who undertakes their treatment. Urinary fistulæs are practically divided into ante-scrotal or urethro-penal, and the scrotal or perineal.

The former, particularly when there is less of substance, so that the opening into the urethra is large, are much the most difficult to close. Sir H. Thompson thus describes them:—"The coverings of

the urethra here are thin, possessing substance insufficient to furnish an amount of granulation adapted to close any but the most insignificant aperture; for the same reason it is difficult to obtain from their immediate neighbourhood a flap endowed with sufficient vitality to preserve its existence after the process of transplantation. And further, owing to the extreme mobility of the member, it is difficult to maintain that perfect steadiness of position so desirable in a part which is the subject of an autoplastic operation; while the alteration in size and form which this organ is especially liable to exhibit, through the occurrence of erections which are often quite uncontrollable, may impair, or sometimes render almost impossible the success of the best planned and most skilfully executed operation."[a] In addition to these difficulties which are peculiar to anterior or penile forms of fistula, must be added the formidable one common to all these cases: viz., the passage of the urine from the urethra through the opening.

The two following cases illustrate each of the two varieties above named, and possess some special points of interest as regards the mode of their production and successful treatment.

Case II.—*Large Urethra-penal Urinary Fistula, produced by a String tied tightly round the Penis—Two Operations.*—J. Doherty, a boy about eleven years of age, was admitted into the Adelaide Hospital in June, 1868, under the care of Dr. M'Alister, then one of the surgeons of that institution. The condition of his penis was as follows:—The prepuce was enormously hypertrophied by the effusion into its cellular tissue of a firm fibrine, evidently the result of former inflammation; a complete phimosis necessarily resulted from this state of the prepuce; the glans penis could be felt about three inches from the end of the elongated prepuce; about half way between the glans and the scrotum a deep *sulcus* or *depression* ran round the entire organ; tracing this to the under or urethral surface, it was found to end in a large opening into the urethra, the size of which may be readily understood from the fact that the top of the little finger of my right hand just filled it up. When the boy emptied his bladder almost all the urine came through this opening.

Dr. Nicholls, of Longford, under whose care this boy had been, obligingly supplied us with information regarding him. The mode

[a] The Pathology and Treatment of Stricture of the Urethra and Urinary Fistulæ. 3rd ed., p. 280.

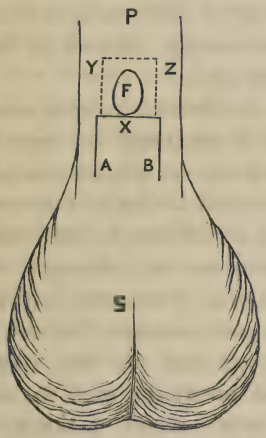
in which this fistula was produced was as follows:—The boy had gone as a patient for scald head into the Infirmary of the Longford Workhouse; while there he constantly wet his bed, and was constantly castigated by the nurse. To avoid this unpleasant result of a habit he could not break himself off, he tied a string tight round his penis one night. Dr. Nicholls being away from home for a few days, was informed by the nurse upon his return that Doherty had a swelling of his penis. Dr. Nicholls, upon examination, noticed a circular indentation, as if caused by a string tied tightly round; the boy stoutly denied having tied anything at first, but afterwards admitted he had done so, but that, finding it hurting him, he had taken it off. The Doctor, put off his guard by this, concluded that the swelling present was the result of inflammatory action, set up by the ligature which continued, although the cause had been removed. He consequently punctured the œdematous prepuce in several places, but in a day or two afterwards found no improvement had taken place. He was then informed by the nurse that the string was still in situ. Upon examination this proved to be the case; the urine was running out, underneath the circular groove, and the end of the string was projecting at the same place; the cord was now cut, but the penis was in such a state that it seemed probable that the whole would slough off; this, however, Dr. Nicholls' care prevented, and the result was the condition as found upon admission.

On account of the hypertrophied state of the prepuce, a permanent phimosis existed. The first step in the treatment plainly consisted in circumcision; this was, therefore, performed by Dr. M'Alister, who subsequently transferred the case to me. The circumcised wound was, at the time I took charge of the case, perfectly healed, and the object of the operation, viz., to disclose the orifice of the urethra, and enable an instrument to be introduced, was perfectly obtained; the urethra took a No. 4 catheter easily. I now made the patient retain his water for some hours, and then pass it in my presence, and as he stood before us it was evident that three-fourths of the stream came through the fistula, about one-fourth finding its way on by the natural orifice, the false opening, I have already stated, was just filled by the tip of the little finger. The first operation, which was performed early in 1869, consisted in making an incision about an inch in length on each side of the opening, parallel to the canal of the urethra, then paring the edges of the opening, and finally bringing them carefully and fully into contact by interrupted sutures, a catheter being retained

in the bladder, and tied in after the operation. The urine flowed through the catheter, altogether for a day or two, but then began to come along the catheter, and make its way out of the wound. When the stitches were removed, upon the fifth day after the operation, the wound was found to have partially united, but on account of the loss occasioned by the paring of the edges, the opening looked as large as it did before the operation. We had, however, the subsequent contraction still to count upon for its further reduction, and, in effect, it was reduced by this means to *one-half* its original size in two months.

During the following summer, when the parts about the fistula had regained to a great extent their natural suppleness, the second operation was undertaken, and performed as follows :—

A flap formed by two parallel incisions (A and B) and by a cross incision (X) connecting them was raised and dissected well back to the scrotum (S). This flap was of sufficient thickness to insure its vitality, about two inches in length, and fully twice the width of the fistular opening it was destined to cover. The next step consisted in refreshing the edges of the opening, which was done by carefully dissecting off from the margin of the opening the fine skin which covered it, and leaving a raw surface squared (Y Z) so as to be fitted to receive the angles of the flap, but the new surface thus made ready, was not as wide as the flap, in order to allow for the contraction of the latter.



The third step consisted in placing and fixing the flap in its new position; it was very readily pushed up, the mobility of the skin in this situation being very favourable, and was found not to be at all too large when fitted, as it had already shrunk considerably. Six points of horse-hair suture were inserted, two at each side, and two at the end; they were easily inserted and held very well. The scrotum was then well supported by means of a broad strap of plaster passed under it and fixed to the abdomen, by which means the flap was maintained in its new position without the slightest dragging on the stitches. A No. 4 catheter was kept in the bladder. The patient lay quietly on his back, and the urine was withdrawn by taking out the plug in the catheter about every four hours. In spite of this, however, a little urine from time to time found its way along the catheter, and out by the side of the flap. The result was that a firm union took place between the flap and the surface where it was placed, except in one spot, where there remained open a passage about the size of a crow-quill; this became finally reduced by the subsequent contraction which always takes place to a mere pin-hole, which, when the boy made water, just allowed a drop of urine to escape slowly while the stream was passing.

The boy subsequently left the hospital, and was well fed and cared for in every way, by charitable persons who befriended him. When I next saw him, which was last summer, the pin-hole oriface remained in the same state. I then endeavoured to close it by means of a probe, heated to redness in a spirit lamp, but without avail. The boy then stoutly refused to have anything further done, so he left hospital again.

The first operation performed in this case was nearly identical with that proposed by Dieffenbach for small fistulæ, and was only preparatory to the subsequent plastic operation, which succeeded much better for the diminished size of the opening obtained by the first operation. It may, I believe, be laid down as a rule, that an ante-scrotal fistula, with less of substance, which is larger than about the diameter of a No. 6 catheter, will require a *plastic* operation to close it; in the performance of which plastic operation the operator must arrange so that the flap will *overlap* the fistula, and that the surfaces to be united shall be *extensive* and *firm*, the flap too must lie easily in its new position, without the slightest straining or dragging.

These points have been aimed at in Dieffenbach's second

operation, also in that performed by A. Alliot, and in that which I performed in this case, which differed from that of the last named surgeon only in his flap being taken from the skin at one side of the fistulæ, and carried across it, while mine was conveniently situated for taking it from the scrotum, the looseness of the skin in this locality favouring this proceeding, and the flap being sufficiently kept in its place simply by supporting the scrotum. This latter point, I believe, materially tended to the success of the operation, as the little flap was thus preserved from any dragging, and no slough whatever took place.

Case III.—*Scrotal Urinary Fistula the result of Abscess—Cure without Operation.*—A young man, who never had a stricture, contracted a gonnorrhœa in November, 1869; this was accompanied by phimosis, and a good deal of constitutional disturbance. After about three weeks, when somewhat better, he imprudently went out skating—got a fall upon the ice, and came home feeling extremely fatigued. That night he had a shivering, and next day was very feverish, with frequent micturition, the urine being bloody. In a day or two, a hard swelling, about the size of a pigeon's egg, was felt in the anterior part of the scrotum. I then saw him for the first time. The constitutional symptoms were of a low type, being accompanied by great prostration; the swelling in the scrotum was exquisitely tender, and evidently contained pus; upon the following day chloroform was administered, and the abscess opened; a thin whey-like pus spirted out; a hot poultice being applied, the patient was much relieved. Upon the following day, I found, as I had expected, that the urine had come freely through the wound when the patient passed water, giving him intense pain. A gum catheter was then introduced into the bladder, and the urine drawn off. A No. 8 passed easily. The patient was instructed to pass this for himself three or four times a day; for two days this did very well, no urine came through the opening, which was beginning to close; but upon the third day, symptoms of irritation of the neck of the bladder began to arise from this frequent use of the instrument, and it had to be discontinued, and the urine again came through the opening; repeated attempts to use the catheter for a sufficient length of time to allow of the healing of the fistula having failed from the same cause, all treatment of this kind was abandoned; the patient was allowed to get up and go out; attention was paid to his general health in every

respect, and he was instructed when passing water to press with his finger upon the track of the fistula, which could be distinctly felt between the external opening and that which communicated with the urethra. By this means only a few drops of urine escaped each time he passed water. Gradually the opening diminished in size, and finally, after about three months, it quite closed; but still when passing water the patient continued to press with his finger, finding that the urine came into and distended a little pouch which communicated with the urethra, but not externally. Finally, this contracted, and he passes water without any symptom of its detention at the seat of the fistula.

In the following case of retained menses from imperforate hymen, the necessary operation was followed by no peritoneal inflammation, although a tolerably free division of the retaining membrane was made. It is interesting, not only on this account, but also on account of the exceptional thick, fleshy character of the septum, and also from the urgent symptoms which existed, and which had occurred a month previously, entirely remitted, and returned with increased intensity at the next menstrual period, the irritability of the bladder making a complete mistake of the nature of the case quite possible.

Case IV.—*Imperforate Hymen—retained Menses—Urgent Symptoms—Operation—Recovery.*—Upon the 7th of January, 1870, I was requested to visit a young lady, fifteen years of age, who, I was informed by the apothecary who had seen her, was suffering from great irritability of the bladder, accompanied with such extreme pain that she had obtained no rest for the last week. She had, it appeared, had a similar attack, only less severe, about a month before the present one. I found a young girl of about fifteen years of age, well developed, but of the dark strumous type of countenance, sitting up in bed, rocking herself backwards and forwards, and writhing from side to side from pain, which she described as a constant dull aching round the loins and hips, and extending round to the lower part of the abdomen; at times the pain increased to an agonizing extent, and then, after a time, subsided again; the urine could not be retained for more than half-an-hour, sometimes was expelled every few minutes, and gave a scalding pain when passing; the pulse was 120. Upon examination of the abdomen with the hand it felt full and hard, but there was no tumour such as a

distended bladder would produce. A No. 5 catheter was then passed into the bladder, and drew off about 2 oz. of pale, healthy urine. In endeavouring to pass the catheter I found the orifice of the urethra pushed out of its normal situation in a direction upwards and forwards, so that it lay in front of the pubis, round which the catheter passed in entering the bladder. Passing my finger in to discover the cause of this deformity, I found the vagina completely closed by some firm, fleshy substance. Without delay a complete examination was then made, and the true nature of the case was apparent. Immediately above the orifice of the urethra the vaginal passage was completely closed by a thick, fleshy, red membrane, which stretched from one wall to the other; it was curved, evidently from the pressure of fluid behind it, and fluctuation could be distinctly felt through it; but the sensation communicated to the fingers was that the membrane which retained the fluid was of considerable thickness. As this examination was made late in the evening, the operation was postponed until the following morning, rest being procured by two quarter grain morphia suppositories.

Upon the following morning, the young lady being placed under the influence of chloroform, I proceeded to divide the imperforate hymen. I found it to consist of an outer portion, which was vascular and fleshy, and which consequently bled freely upon incision, and an inner, which was very tense, bluish in colour, and membranous in appearance. The thickness of the outer part, which was distinctly seen by the depth of the incision before the inner bluish layer was reached, was, I think, fully the one-eighth of an inch. The moment a small opening was made with the point of the knife through the inner part of this thick septum, a jet of a brownish-black fluid spirted out; a director was then introduced through the puncture, and upon this a blunt pointed bistoury, by which the opening was enlarged *a little* both forwards and backwards. This was followed by a very copious discharge of the same dark treacle-like fluid. Immediate relief of the distressing sense of bursting and bearing down pain followed the operation; the patient was removed to bed, kept lying on her back, with napkins constantly renewed under her, and a warm sponge applied to the vulva.

The imprisoned menstrual fluid continued to discharge itself for about twelve hours; it then ceased to flow; but upon any movement of the patient recommenced for a time. Upon the following day I found the patient had passed a very good night; she stated she had no pain; the irritation of the bladder was entirely gone. Upon

examination I found the situation of the obstructing membrane to be much higher up than appeared to be the case before the operation; it was found now to be fully half-way up the passage of the vagina.

Next day, forty-eight hours after the operation, the edges of the opening made in the septum were found, upon examination, to have partially united together, leaving only a small aperture still free, through which the same dark-coloured fluid was occasionally discharged. The patient was at once placed in a good light, and the edges of the flaps carefully separated from one another, which was readily effected with a director and the handle of a knife. A tent of oiled lint was then introduced well into the vagina, the orifice of the urethra being left free. Directions were given that the patient was strictly to remain in the recumbent posture.

Two days after this I found that the lint had remained in until the evening before, when, upon the bowels acting, it had slipped out. The opening was now sufficiently free, admitting the top of the index finger; the nozzle of an india-rubber enema bag was then introduced, and the cavity of the vagina was freely washed out with warm water. The patient, being perfectly free from any symptoms of inflammation, was allowed up. Directions were given that the vagina should be injected daily, which was carefully carried out by the patient's mother. At the next menstrual period the discharge appeared and escaped freely, and was healthy in all respects, and the young lady continues well to the present time.

The operation of ovariotomy is now fully recognized as a legitimate one in suitable cases. It has been the means of saving many lives, and is very naturally looked upon with pride not only by the able surgeon to whose energy its restoration is due, but by the whole profession in these countries. Yet it is an operation so beset by dangers that it seems to me advisable that fatal as well as favourable cases should be recorded as they occur. In the following case the diagnosis was fully confirmed in every particular; the patient was of favourable age and temperament, healthy in every respect, except her terrible disease. Difficulties which had been to a great extent foreseen occurred during the operation, and were successfully encountered; yet rapid peritonitis followed, extinguishing life in twenty-four hours.

Case V.—Mrs. B——, thirty-six years of age, the wife of a policeman, applied to me in May, 1870, for advice regarding the

swollen state of her abdomen. She had two children, the youngest being eight years old. About two years before her application for advice, she noticed a tumour, about the size of a small orange, low down in the left side of the abdomen. This has steadily increased ever since. Having satisfied myself that the tumour was not a pregnant uterus, and that it probably was ovarian, I advised the patient to come into hospital. She consented, and when there I had the advantage of the opinion of my colleagues upon the nature of the case. Dr. Kidd also carefully examined the case at my request, and concurred with my colleague, Dr. Atthill, in stating that the uterus was healthy. The signs which were noticed, and the diagnosis which was arrived at, were as follows:—The abdomen measured thirty-six inches in circumference; it was, both anteriorly and laterally, quite dull everywhere upon pressure; far back upon each side the colon could be found. A small protuberance existed beside the umbilicus, which was increased by coughing, and seemed to receive an impulse; it could not be reduced. The whole tumour was more fixed than a free ovarian tumour would be; its surface was unevenly lobulated, the greater part, particularly that in the left side, feeling very hard and solid; while the upper portion and that to the right side was evidently fluid. Taking all the signs and circumstances of the case into consideration, the diagnosis we arrived at was, that we had to deal with a multilocular ovarian tumour which had rather extensive adhesions to the abdominal wall. The small protuberance beside the umbilicus was supposed to be omental. Ovariotomy offered the only hope of relief, but before deciding upon the operation we agreed to wait and see if the tumour was growing, and if so at what rate.

Upon the 25th of May, one month from the date of the former measurement, the circumference of the abdomen was found to be 40¼ inches; thus the tumour had increased 4¼ inches in one month. This rapid increase coincided with the sensations of the patient. She said she could no longer walk even a short distance, and was greatly oppressed with the weight and pressure of the tumour. Under these circumstances, the nature and dangers of the operation were explained to her. She embraced the hope of life by means of the operation at once, and almost compelled her husband to give his consent. Tuesday, the 24th of May, at nine o'clock, was fixed upon. I was ably assisted by my colleagues,

Drs. Walsh, Richardson, Head, Little, and Atthill, also by Dr. Kidd, who had already lent his valuable aid in the examination of the case.

The patient having been placed fully under the influence of chloroform, I pinched up the skin into an even fold midway between the umbilicus and pubis, and by transfixing this made an incision about $3\frac{1}{2}$ inches in length, which was subsequently increased by about another inch, so that the opening as finally made was $4\frac{1}{2}$ inches. As soon as the skin had been divided the cellular membrane or fascia was carefully slit up upon a broad director layer after layer. The rectus muscle came into view, and some fibres of it were divided. Very dense fascia then presented itself, and was slit up. Immediately after this, whilst cautiously proceeding, a gush of a thick (evidently ovarian) fluid announced that a cyst had been opened. The finger being immediately placed in the opening, the further escape of fluid was prevented. It became evident that the peritoneal cavity was quite obliterated by adhesions at its lower and anterior part. The fingers, pushed in carefully along the cyst which had been opened, were found capable of breaking down the adhesions, which were found to extend all across from one iliac fossa to the other, but not higher than the umbilicus; downwards they extended a little way behind the pubis. When all adhesions had been broken down, the cyst which had first appeared was pressed into the incision and tapped. The fluid, which was dark in colour, and the consistence of glycerine, passed readily through the canula and tube. When it was emptied another was pressed up and seized in a strong vulsellum; the trocar being withdrawn from the emptied one was thrust into the second, which was readily accomplished, and the sac held as usual by the strong claws attached to the trocar; but no fluid escaped through the canula. The trocar had evidently penetrated the cyst, the fluid contents of which could be readily recognized through its protruding walls. Why no fluid escaped seemed strange. However, there was no time lost in withdrawing the trocar, and then there slowly flowed out a thick, whitish semi-fluid substance like starch; it was too thick to flow through the canula. Very firm pressure upon either side of the wound prevented any of this substance, thus slowly trickling from its cyst, from entering the cavity of the abdomen. There was not any very large quantity of it, and it was safely removed. The hand was now

introduced and passed round the remaining mass. No adhesions were encountered, but the mass was still much too long to pass through the opening. This was then somewhat enlarged. Another large cyst was with some difficulty brought to the opening and tapped; the fluid from this flowed readily, being of a light amber colour, like urine. When this was accomplished the remainder of the mass was safely delivered through the opening with the aid of the hand introduced behind it. The pedicle was found to be sufficiently long to admit of its being safely secured outside the wound. This was accordingly done, and the diseased mass cut off. The pelvic cavity was now carefully cleansed from blood by warm new sponges. The left ovary was found to be of natural size. Then the wound was closed with silver wire sutures. Lint soaked in carbolic oil was laid over the wound, the whole being covered with cotton wadding and a flannel roller. The patient was then lifted into a well warmed bed. She was still partially under the influence of chloroform. Her countenance was deadly pale, but her pulse was good. She vomited a very little immediately after being placed in bed. Some ice was kept in the mouth, and a half-grain morphia suppository was placed in the vagina. In two hours after the operation her pulse was 80 and full; skin warm; the stomach quiet. She complained of great pain in the wound, and a general feeling of uneasiness. A second half-grain morphia suppository was given. Two hours after this her pulse was 100; her skin was hot; she was thirsty; complained still of pain in the abdomen; had asked for and drank some cold beef-tea; no sickness of stomach. Twenty grains of hydrate of chloral was given and repeated in two hours; but the restlessness continued to increase as the evening advanced; severe pain in the wound and abdomen was at times complained of. She would fall for a few minutes into a heavy sleep, wake up suddenly crying out with pain, and then dose off again. At ten o'clock the pulse was 140 and weak; the skin was burning, the arms were constantly tossed restlessly about, while a ceaseless complaint of pain in the abdomen was made. During the night she was continually raving and throwing herself about, complaining at times of hunger. She drank freely of beef-tea and brandy and water. She died at eight o'clock the following morning, twenty-three hours after the operation. The *post-mortem* examination showed the cause of death to be acute peritonitis; the peritoneum, both visceral and parietal, over the

lower half of the abdomen, was everywhere covered with soft recent lymph; the surface where the adhesions had existed looked very vascular and rough; there was about a pint of bloody serum in the pelvis; the pedicle remained perfectly secured by the clamp, and did not appear to have dragged injuriously upon the uterus.

Examination of the tumour showed it to be composed of a vast congeries of cysts. It contained no solid mass whatever. The parts which before the operation gave the signs of solidity were now seen to be composed of a multitude of cysts growing one within another. The three cysts which had been tapped during the operation were the largest, containing a quart or more of fluid each. From the inner wall of each of these cysts there projected several tumours, each composed of a group or cluster of cysts, which secondary cysts were each about the size of a grape. Some of these contained a viscid glairy fluid; others had much thinner, more watery contents.

The diagnosis in this case was verified in every particular; yet the operation proved rapidly fatal. In considering why this was so we may note the four following circumstances, which seem in this instance to have increased the risk which always accompanies the operation:—

1st. *The extensive adhesions* which glued together the peritoneal surfaces throughout the anterior and lower part of the abdominal cavity, although so soft as to be readily broken down with the hand; yet no doubt directly tended to produce the fatal peritonitis.

2nd. *The multilocular character of the tumour.* This rendered the operation more tedious than it would have been with a single cyst, and the danger of the contents of some of the cysts getting into the abdomen was much increased by the withdrawing of the trocar from one to evacuate another cyst.

3rd. *The extreme viscidity of the contents of one large cyst.* This not only still further delayed the completion of the operation, but the danger of peritoneal irritation from the entrance of this fluid into the cavity of the pelvis was much increased.

4th. *The presence of a piece of omentum adhering to the tumour and projecting through the umbilicus.* This not only gave rise to

some hæmorrhage, but was of necessity much handled in the removal of the tumour, and in the subsequent cleansing of the peritoneal cavity, and thus formed another source of danger.

The fact of each or all of these sources of danger being present in any case, should, I conceive, make the surgeon weigh anxiously the rate of progress, and other circumstances of the case, to determine whether, palliative treatment might not be better than incurring the great risk of the operation. In such a case, however, as the one I have now detailed, where the increase was rapidly going on, the powers of the patient were beginning to fail, and where no palliative treatment was available, it was right to attempt to save life by the only means possible.

ART. V.—*Is the Flesh of Diseased Animals Wholesome Food?* By CHARLES A. CAMERON, Ph.D., M.D., L.K. & Q.C.P., Professor of Hygiene in the Royal College of Surgeons; Lecturer on Chemistry in Steevens' Hospital, Medical College, and in the Ledwich School of Medicine; Analyst to the City of Dublin.

THE animals whose carcasses constitute the most abundant source of the animal food consumed in these countries are liable to diseases almost as numerous as those which affect mankind. Many of the maladies of the domesticated animals are produced by poisons, probably varieties of the *microzymæ*, which play such havoc amongst the human species, and are each of them specific entities. The contagious diseases of the lower animals are termed epizootics, and in their mode of propagation they resemble in every essential respect the zymotic diseases which afflict the human race. That some epizootics are capable of becoming zymotics, we have a familiar and continuous proof afforded in the conversion of bovine variola into human vaccinia; and even when the virus of an epizootic is incapable of producing its characteristic symptoms in man, it still in many cases develops some form of disease which occasionally terminates fatally.

The following epizootics are those from which the farm animals of these countries have most suffered:—The rinderpest, or con-

tagious typhoid fever, principally affects oxen, but occasionally the sheep suffers from it. It is endemic in the southern parts of Russia, and would now be endemic in Great Britain but for the vigorous measures adopted for its extirpation when it broke out in these countries five years ago. Contagious lung distemper seldom affects any animal save the ox. The ox and sheep suffer from small-pox, but the pig appears to enjoy exemption from this complaint. A disease popularly termed "the soldier," which resembles scarlet fever in man, is often endemic in Ireland. It is a very fatal affection. The poison of anthrax affects cattle, sheep, and swine, and is occasionally a great scourge. The so-called splenic apoplexy (the *maladie du sang* of the French veterinarians) of oxen; the "black leg" or "quarter evil" of calves and young cattle; the glossanthrax of oxen and pigs; the "braxy" of sheep; the carbuncular cynanche, or anthracoid angina, the hæmorrhoidal anthrax, and the gangrenous erysipelas of pig, and the malignant boil which attacks the latter animal's throat—all are forms of carbuncular fever.

Panzootic aphtha affects all the domesticated animals. In Dublin we recently had an outbreak of this malady amongst the dairy cows. Eczema epizootic, or foot-and-mouth disease, chiefly ravages our herds, but it often attacks our flocks. It is one of the least fatal of the epizootics, but perhaps the most contagious. The disease is an eruptive fever, and its characteristic symptom is a vesicular eruption on the mucous surface, more especially that of the mouth and adjacent parts. The eruption also appears on some of the parts uncovered with hair—the udder and between the digits, for example.

Parasitic diseases of the domesticated animals are common. "Sturdy" is occasioned in the sheep by the presence of a bladder worm (*Cœnuris cerebralis*) in the brain. The "rot" in cattle and sheep is caused by the presence of very large entozoa, termed flukes (*Distomæ hepaticæ*), in the animal's liver; but there is no evidence to prove that the flukes found in the liver of sheep or oxen find their way into the livers of men.

The disease termed measles in pigs is produced by the presence of great numbers of *Cysticerci cullulosæ*. They are found in all parts of the animal's flesh, but they are most abundant just beneath the skin, on which they elevate what we may term measle mounds. The worms are found in bags which are situated between

the muscular fibres. Meazly pigs are distinguishable by the protuberances on the inner surfaces of their eye-lids, and on the under surface of the tongue, by a yellow speck in angles of the eyes, and by their thickened neck and thin loins. Meazly pigs are very common in Ireland, much more so than in either England or Scotland. The oxen also suffers from a species of measles.

I think there is little doubt but that the pork measle is an immature tape-worm, and I believe that when it passes alive into the human body it becomes developed into that human entozoon Thorough cooking undoubtedly destroys the measle-worm; but pork sausages are sometimes eaten when they are far from being completely done. I have reason to know, too, that meazly pork is very likely to be employed in the preparation of sausages; and it is not a pleasant reflection to think, when discussing this savoury kind of meat, that we are possibly introducing into our body the embryo of a tape-worm ten yards long!

The *Trichina spiralis* is the immature condition of a very little nematode worm. The sexually developed trichina is found in the intestinal canal of a great variety of animals. Here the eggs of the entozoon are produced, and from them issue the embryo trichinæ in the form of minute threads. The painful and often fatal disease termed trichiniasis is caused by the trichinæ penetrating the abdominal wall, and wandering in vast numbers throughout the muscular structures. In about a fortnight the trichinæ attain their full size, and after disorganizing more or less completely the fibrillæ and sarcolemma, they become encysted by calcareous matter.

When meat containing encysted trichinæ is swallowed, the gastric juice dissolves the calcareous cysts, and sets free their dangerous tenants. *Thorough* cooking destroys the trichinæ; but that ordinary boiling and frying do not always render these parasites innocuous is evident from the hundreds of cases of *trichiniasis* which have been recorded within the last twenty years. Two cases of trichiniasis in the human subject have come under my own observation in Dublin; but although I am constantly on the look-out for the trichinæ in Irish pork and bacon, I have not as yet detected it.

There is considerable diversity of opinion relative to the amount of injury to human health and life occasioned by the use of the flesh of animals slaughtered whilst in a diseased condition, parasitic maladies being excluded. If we adopt the opinion of the distin-

guished French writer on hygiene, Parent du Chatelet, the human stomach is capable of digesting, without injury to the system, the most highly diseased and putrid kinds of flesh. During the last great outbreak of rinderpest in England and Scotland, thousands of oxen, killed whilst affected with the disease, were eaten, and it is asserted that no bad results followed. In Ireland, where contagious lung distemper has been prevalent since the year 1841, the carcasses of immense numbers of animals affected with that disease must have been used as food, and yet we do not know of any serious consequence to human life or health having resulted therefrom. It must, however, be admitted that until quite recently very little attention has been devoted to the subject of the influence on health produced by the use of the flesh of diseased animals. Perhaps the consumers of the beef furnished by cattle affected with pleuro-pneumonia have not enjoyed that immunity which stock-feeders, salesmasters, and butchers maintain has been the case. A man taken ill after dining upon a beefsteak not obviously unsound would seldom attribute his indisposition to the use of diseased meat. Negative testimony is not, however, worth much in most cases, and that perfectly fresh meat is sometimes capable of producing disease, and even of causing death, is a fact of which there is abundant positive testimony.

Professor Gamgee states that several cases of illness produced by the use of the flesh of animals suffering from inflammatory diseases have come under his observation. Dr. Livingston alleges that in South Africa the negroes and Europeans who eat the flesh of oxen affected with pleuro-pneumonia suffer from malignant anthrax; and it is a significant fact that since the introduction of this epizootic into England, the mortality from carbuncle and phlegmons generally has steadily and largely increased. During the five years before the appearance of the lung distemper the annual deaths from phlegmons averaged 5 per 20,000 deaths from all causes; whilst during the ten years ended in 1856, the mortality from these diseases rose to 162 per 20,000 deaths. The Registrar-General for Scotland states that the mortality from carbuncular diseases has greatly increased in that country since pleuro-pneumonia became an endemic disease amongst its herds. It is not within the scope of this paper to refer to the numerous writers who have given us accounts of attacks of disease produced by eating meat apparently sound, nor on the other hand to quote

from the writings of authors whose experience would seem to indicate that the use of the flesh of diseased animals rarely if ever causes human maladies. In the works of Parkes, Levy, Londe, Devay, Motard, and other systematic writers on hygiene, this vexed question is fully discussed. My object in reading this paper is to publish my own experience—though it is not very extensive—in connexion with this interesting subject, and also to describe the system of police and sanitary inspection of food which has recently been established in Dublin.

Until the year 1867 there was no systematic supervision of the animal food on sale in this city. Occasionally the Lord Mayor, accompanied by his officers, paid a brief visit to the markets, and if a piece of putrid meat happened to attract their attention it was seized and confiscated. I feel happy in having been instrumental in originating and organizing a very efficient system of inspection of the meat, fish, and fresh vegetables on sale or in preparation for sale in Dublin. The manner in which I succeeded in accomplishing this matter I need not describe. Four constables of the metropolitan police, in uniform and subject to police discipline, are employed for the sole purpose of inspecting the slaughter-houses, butchers' stalls, cattle, fish, fruit, and vegetable markets, and all other places where meat, fish, fruit, and vegetables are to be found. These officers are paid by the Corporation, and act directly under a "Deputy Clerk of the Market" (the Lord Mayor being Clerk of the Markets), whose time is almost wholly occupied as an inspector of food. These five officers are commissioned as inspectors of nuisances, which, under the Sanitary Act of 1866, gives them the right of entry into certain premises, and the power to seize food if they suspect it to be diseased or otherwise unsound. Eight sergeants of the metropolitan police are employed on sanitary work by the Corporation, and these officers often detect unsound provisions. The Commissioners of Police, and more especially James Moore O'Ferrall, D.L., the senior commissioner, have always shown the strongest desire to stamp out the infamous traffic in diseased meat. Without their co-operation such a task were an impossibility. Lastly, the whole police force, comprising more than 1,000 men, have directions from the Commissioners to aid the special constables employed as inspectors of food. For example, when a sick animal or unsound carcass is detected, an ordinary policeman is placed in charge of it until it is disposed of. Whenever there is

the slightest doubt as to whether food under seizure is sound or not I inspect it, and either pronounce it sound or give a written certificate stating that it is unfit for human food.

Before the official inspection of meat was organized in Dublin, the usual practice was to send up to Dublin the carcasses of cows, sheep, and pigs that died or were killed whilst diseased, within a radius of at least twelve miles from the city. Every dairy cow that had pleuro-pneumonia, or that died or was killed during parturition, found its way into the shambles. It is a fact that the Dublin knackers formerly rarely had the carcass of a cow sent to them, whilst now not a day elapses without one or more head of horned stock being consigned to them. The most extraordinary devices have been resorted to in order to baffle the vigilance of the police. Diseased carcasses have been conveyed into the city in almost all kinds of vehicles that would be likely to elude observation. On one occasion a hearse was employed, but its contents—two bad carcasses of beef—were discovered. During the present year the quantity of diseased and putrid meat confiscated in Dublin exceeded 50,000 lbs. per month, or at the rate of 400,000 pounds weight per annum.[a] In no other town in the United Kingdom has there been anything approaching to so large a quantity detected; indeed, as a general rule, there is an untrammelled traffic in unsound food carried on in nearly all parts of these islands.

The power to carry out a compulsory inspection of meat and other kinds of food is entrusted to certain local authorities—corporations, town councils, boards of guardians of the poor, &c.—by the Nuisances Removal Act, 1863, which is incorporated with the Sanitary Act of 1866 (29 and 30 Vict., cap. 90). Some doubt having arisen as to the extension of the clause of that Act relating to food to Ireland, the point was cleared up by a short Act of Parliament, passed (32 and 33 Victoria, cap. 108), at the request of the Public Health Committee of the Corporation of Dublin. The following is the clause relating to food inspection, from which it will be seen that the authorities have power to seize any kind of diseased meat:—

"2. The Medical Officer of Health or Inspector of Nuisances

[a] Since the foregoing was written, the exact quantities confiscated during the year 1870 has been ascertained. They amount to the following quantities:—Beef, 364,400 lbs.; veal, 1,500 lbs.; mutton, 2,100 lbs.; hams and bacon, 9,339 lbs.; pork, 8,150 lbs.; venison, 150 lbs.; fish, 2,741 lbs. Total animal food, 388,380 lbs.

may at all reasonable times inspect and examine any animal, carcass, meat, poultry, game, flesh, fish, fruit, vegetables, corn, bread, or flour exposed for sale, or deposited in any place for the purpose of sale, or of preparation for sale, and intended for the food of man, the proof that the same was not exposed or deposited for such purpose or purposes, or was not intended for the food of man, resting with the party charged; and in case any such animal, carcass, meat, poultry, game, flesh, fish, fruit, vegetables, corn, bread, or flour appear to him to be diseased, or unsound, or unwholesome, or unfit for the food of man, it shall be lawful for such Medical Officer of Health or Inspector of Nuisances to seize, take, and carry away the same, or direct the same to be seized, taken, and carried away by any officer, servant, or assistant, in order to have the same dealt with by a justice; and if it shall appear to the justice that any such animal, or any of the said articles, is diseased, or unsound or unwholesome, or unfit for the food of man, he shall order the same to be destroyed, or so disposed of as to prevent such animal or articles from being exposed for sale or used for such food; and the person to whom such animal, carcass, meat, poultry, game, flesh, fish, fruit, vegetables, corn, bread, or flour belongs, or did belong at the time of sale or of exposure for sale, or in whose possession or on whose premises the same is found, shall, upon conviction, be liable to a penalty not exceeding twenty pounds for every animal, carcass, or fish, or piece of meat, flesh, or fish, or any poultry or game, or for the parcel of fruit, vegetables, corn, bread, or flour so found, or at the discretion of the justice, without the infliction of a fine, to imprisonment in the common gaol or house of correction for a term of not more than three calendar months."

Since this Act has been brought into operation in Dublin several convictions under it have been obtained. Persons have been imprisoned for periods varying from one to three months, and others have been fined sums of from £1 to £20, together with costs. The results of these convictions have been most satisfactory. Many butchers who had been notoriously engaged—some of them solely—in the diseased meat traffic, have utterly relinquished their nefarious business. Inspection of diseased animals, formerly shunned, is now often courted; for in some cases the inspected carcasses are allowed to be sold, when only

slightly affected, for food purposes, or boiled down, in order to extract "chandler's fat" from them. I may safely state that the citizens of Dublin are now almost perfectly certain to get the flesh of sound animals in every butcher's shop in the city.

As I have so much experience in relation to diseased meat, it may be presumed that cases of illness produced by its use occasionally come under my notice. Such is the case. Persons call on me from time to time to complain that they or members of their families have been made ill by eating what they believed to be diseased meat. Sometimes they bring a portion of the offending article. This occasionally is semi-putrid, but more frequently it is fresh; in the latter case it generally possesses a sickly cadaverous odour. Mealzy bacon is often complained of; and I have seen several persons who referred severe illness, which they were suffering from, to the use of bacon or pork containing the *Cysticercus cellulosus*. In July last I inspected a cow's head, a portion of which had been eaten by a family, all of whom shortly after were affected with retching and diarrhœa. The part of the head that remained was completely devoid of any odour approaching to that of putridity, but it emitted a loathsome odour, resembling that so often smelled in the chambers of the sick. It appears that the fleshy portion of the head had been in great part removed from the skull, and broiled on embers, which probably only imperfectly cooked it. A strong man—a labourer in a timber yard—his wife and children, partook of the meat, with the simple addition of bread; the man eat heartily, but the woman and her children only used a small portion of the meat. Less than an hour after their dinner the whole family became unwell, with diarrhœa and vomiting, but, with the exception of the man, recovered in less than twenty-four hours. The husband, however, remained in bed seriously ill for nearly a week. I happened to see him thirty-six hours after his unlucky meal, and found him much prostrated. His tongue was very foul, his pulse feeble, and he particularly complained of pain, accompanied by a sickening sensation, in his stomach. For nearly two days he had very bad choleraic diarrhœa. I carefully examined a portion of the cow's head, but I found no evidence of parasitic disease.

In August, 1869, I visited a family, consisting of six persons, all suffering from choleraic diarrhœa, which they could only attribute to the use of some chops which had constituted the staple item of

their dinner. On inquiring closely into the facts of the case, I came to the conclusion that the surmises of the family as to the cause of their complaint was correct. A portion of the chops had remained, and although quite fresh, it had the disagreeable sickly odour which I have so frequently observed in the case of the flesh of animals obviously diseased.

During the last three years more than a dozen persons have been summoned before the Lord Mayor and the police magistrates for having sold meat which witnesses swore in court had made them unwell. In every case I carefully inspected portions of the meat complained of, and found that it was generally more or less putrid, or that it possessed a rank odour, or that parasites were present in it. In three or four cases, however, the meat did not present any external appearances of disease. Last summer an inquest was held on the body of a man who died in Sir Patrick Dun's Hospital, and the verdict found by the jury was that the deceased had come by his death in consequence of his having eaten diseased bacon. The medical evidence given proved that the trichina spiralis was not the cause of death.

The disease which is most fatal amongst oxen in this country is contagious pleuro-pneumonia. There are two forms of bovine pleuro-pneumonia—one resembling that disease in man, and non-contagious; the other a highly contagious and much more fatal malady, imported into this country from the continent in 1841; it is now unfortunately completely domiciled here. The usual symptoms of contagious pleuro-pneumonia are very marked. The attack is generally ushered in with shivering; next a slight cough is heard, which gradually increases in severity as the disease progresses. The pulse is full, and ascends from about 70 to over 100; but ultimately it becomes weak and indistinct. The temperature increases, and the symptoms of acute fever present themselves. In the severe stage of the disease there is usually a very loud respiratory murmur; and at each inspiration the nasal cartilages rise spasmodically. Pressure on the spine produces great pain. The eyes become bloodshot, and foam often issues from the mouth. By means of the stethoscope the serious structural changes which the lungs undergo are readily determined. The dry sonorous râle of ordinary bronchitis is observed; and a sound as if pieces of leather were undergoing friction may be observed until the last stage of the disease over a large portion of

the thoracic surface. There is often diarrhœa. The *post-mortem* appearances are striking. There are extensive adhesions between the costal and visceral pleuræ. The trachea contains large quantities of false membranes. The lungs are enormously increased in size, and are frequently hepatized to the extent of three-fourths of their volume. There is great destruction of lung tissue. The pulmonary lobules are reddish-brown, and being cemented by layers of dense and lighter coloured lymph, the whole mass often presents an appearance resembling that of Sienna marble. The quantity of purulent matter infiltrated through the lungs and occasionally accumulated in cavities and abscesses is sometimes very large. I have seen more than a gallon of purulent matter flow from the lungs of a single cow. The lungs often attain to an enormous weight. In their healthy condition their average weight is about 12 lbs.; in advanced stage of pleuro-pneumonia they generally weigh from 30 to 70 lbs. On one occasion I found them to weigh a little over 100 lbs. The lungs which I exhibit to you weigh more than 5 stones. From my experience of this disease, I believe it generally t rminates fatally.

The flesh of well-fed oxen, killed whilst affected with pleuro-pneumonia, varies very much in appearance. In the congestive stage of the malady the flesh is undistinguishable from that of healthy oxen. In the latter stages of the disease, and immediately after death, it frequently, but by no means invariably, presents a somewhat abnormal appearance; but on the following day it is not difficult—at least to the initiated—to distinguish it from healthy beef. Good meat possesses a dull red hue and a marbled appearance. It is firm to the touch, and the texture is less elastic the day after the death of the animal that furnished it. Its odour is not disagreeable. On the contrary, the flesh of diseased animals is more flabby the day after they are slaughtered; it is rather plastic and moist, and its membranes exude a glairy liquid. On the first day the colour is generally preternaturally red, but on the second, and especially the third day, it acquires a dark brown shade. Dr. Letheby says that in diseased meat the juices, instead of being, as in the case of healthy flesh, acid, are often either neutral or alkaline. I have never found this to be the case until at least twenty-four hours after death, when probably it was caused by ammonia being liberated from the decomposing blood—for diseased meat decays very rapidly.

The milk of cows affected with pleuro-pneumonia does not appear to have been much investigated. I find that it generally contains a high proportion of solids; and in two instances, where the cows were actually dying, I found bactera; and vibriones in their milk—a clear indication that retrograde metamorphosis of matter was taking place in it. The losses sustained by the Dublin dairymen, in consequence of the ravages amongst their herds, is very great. So far as I can learn, during the last year at least 8 per cent. of their stock suffered from the disease. The dairymen cannot be convinced that the disease is contagious, and therefore, unless under compulsion from the sanitary authorities, they never disinfect their premises after the removal of diseased animals from them. In many cases, too, the vital powers of the cows are lowered by their constant respiration in close and fœtid stables. In some of the sheds the cubic space allowed to a large cow is even less than the *minimum* 300 cubic feet of breathing room allowed to a man in the registered lodging-houses. I once saw a large number of dairy cows so close to each other in a dairyman's stable that it was quite impossible they could all lie down together. On questioning their owner on this point, he facetiously replied, "Gorrah, sir, they take it turn about!"

It is urged by those who assert that the flesh of diseased animals is, except under very exceptionable circumstances, wholesome food, that neither the chemist nor the microscopist has as yet discovered any specific poison in it, except such parasites as trichina spiralis. It must, however, be borne in mind that Lionel Beale has vainly sought for the contagium of diseases by means of a microscope so powerful that if it were possible to look at a man through it he would appear to be as tall as Chimborazo. With this instrument he could only discover as abnormal a little granular matter in the blood taken from a cow affected with rinderpest; and he could discover no differences between the cells which composed a malignant tumour and those of healthy flesh—between human healthy lymph and vaccine lymph. It is not unreasonable, therefore, to demand that before the flesh of animals suffering from loathsome diseases is prohibited as an article of food, the chemist, or the microscopist, or the pathologist, shall demonstrate that it contains an isolable poison or virus? The purchasers of butchers' meat would not buy the flesh of an animal that they knew had such a disease as black-leg or lung-distemper; and should there be

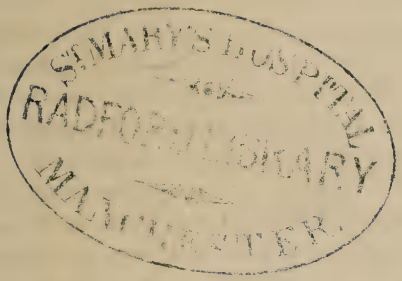
even a slight doubt as to the unwholesome nature of such food, it is clearly the duty of the sanitary authorities to place an interdict upon its sale in open market.

With respect to the milk of animals suffering from inflammatory complaints, there is sufficient evidence to prove that at least, in the case of foot-and-mouth disease, it occasionally produces bad effects. A series of several cases of what may be termed *herpes contagiosus* have been referred to the use of the milk of cows affected with foot-and-mouth disease. In the annual report of the Medical Officer of the Privy Council for 1869, recently published, Dr. Thorne shows clearly that a disease is sometimes produced in the human subject when the milk of cows suffering from foot-and-mouth disease is freely used without being boiled. Last year foot-and-mouth disease prevailed extensively for about two months in Dublin. The fatality from it was not high—probably 2 or 3 per cent. I made careful inquiries as to the effect which the milk of some of the affected animals produced on the health of those who drank it, but I could not get much satisfactory information. In one instance only did I ascertain that the milk of cows seriously affected with the malady probably produced disease. Three children, who largely consumed the milk, both cooked and uncooked, suffered very much from aphthous patches in their mouth and on their lips. In the case of pigs, there is the strongest evidence that several of these animals died in consequence of being largely fed upon the milk of cows suffering from epizootic aphtha.

ART. VI.—*Case of Bronzed Skin (Melasma), without Disease of the Supra-Renal Capsules.* By WILLIAM MOORE, M.D., Trin. Coll., Dublin; King's Professor of the Practice of Medicine; Professor of Clinical Medicine, and Physician to Sir P. Dun's Hospital; Physician to the Institution for Diseases of Children, &c., &c.

SARAH DOWDALL, aged sixty-two, was admitted into Sir P. Dun's Hospital in April last, under the care of Dr. Aquilla Smith, and on the 1st of May she was passed over to my care.

Her father died of some "paralytic" disease, and her mother died of pulmonary phthisis, at twenty-eight.


ST. MARY'S HOSPITAL
MANCHESTER

This patient had always been pale, slight, and delicate, but as her circumstances during her husband's lifetime were "easy," her health was moderately good. She had borne ten children, six of whom died young, two died of phthisis, and two are alive and healthy. She always lived in Dublin or the neighbourhood, and had no "coloured" relations.

On coming under my care she presented the following appearance:—Her face generally was of a dark brown olive colour, whilst the lips and mucous surfaces of the eyelids were anæmic, and the sclerotic coat of the eyes of a "pearly blue colour, the pupils being minutely contracted." There were brownish discolourations on the right half of the tongue, and on the right buccal membrane. The skin on her neck was as dark as an infusion of strong coffee, but it became absolutely black in the axillæ and over the chest, abdomen, groins, pudendum, and inside the thighs. The arms, hands, and legs generally were of an olive brown colour, and the front of both tibiæ were darkly mottled. The skin had a peculiarly soft greasy feel, and the cuticle was peeling off the body and arms in parts. There was a most disgusting odour or emanation from the patient, which the nurse described as "sickening;" it may have been similar to that exhaled from people of colour, but I am not able from experience to state this with confidence. From this description it will be readily seen the patient might have been mistaken for a "mulatto." Her daughter stated that for the past seven or eight years her body from being pale had gradually been getting discoloured, the extremities being the last to take on the discolouration; for the past two years she had been getting emaciated, and when she came under my care she was reduced to a skeleton, and her appetite was quite gone. Her asthenia was extreme, and she was always in a drowsy state: to speak was a great effort to her, and then it only amounted to a well developed whisper, and gradually the asthenia increased till she gave up speaking almost altogether. A short time before she came into hospital her mind seemed to be giving way, and when I first saw her she was quite "demented," her delusions being of a quiet, passive, romancing order, which continued at intervals till her death.

A physical examination of her chest showed dulness over the apex of the right lung, where a dry râle could be heard; the heart's sounds were so faint as scarcely to be heard, and eventually they became "single."

Dr. Finny was kind enough to make a careful analysis of the

urine of this patient, which had an acid re-action; a specific gravity of 1014, and contained neither sugar or albumen; in short, it presented nothing abnormal. My colleague, Dr. Thomas Little, found an excess of white corpuscles in the blood, but whether it was a relative or absolute excess, he was not prepared to state.

This case resisted all medicinal and stimulating treatment; in fact, the only thing the patient would take was "porter," of which she managed to sip about a pint in the twenty-four hours, which was in great part vomited, or more properly regurgitated. Things went on from bad to worse, and she became daily weaker and more drowsy, and finally, as if slept away, having made no complaint of pain from first to last.

In making the *post-mortem* examination I had the able assistance of my colleagues, Drs. Bennett and Little. The body generally was greatly emaciated; the heart was normal in size, and covered with a goodly amount of fat externally, but there was no fatty degeneration.

The liver was pale in colour but healthy, the spleen was normal in size and healthy; there were some peritoneal adhesions, and the glands of the abdomen generally, viz., the mesenteric and lumbar, were masses of tubercle, whilst a small cavity existed in the apex of the right lung, and the left was studded with tubercle.

The brain showed no evidence of disease; the kidneys were normal, and the capsules of a pale yellowish colour; the left slightly larger than the right, but neither of them enlarged or thickened in any way; their length did not exceed an inch and quarter, whilst their width was somewhat less. A section showed the normal cortical structure (without any adventitious deposit of any kind) with the dark brown central part. Large branches of the semi-lunar ganglion entered the capsules on their upper part, but not larger than those off-sets from the ganglion which are usually seen, nor larger than we could expect from the immediate proximity of these bodies to the ganglia.

A microscopic examination of the capsules showed fine tubules in the cortical structure, which, as far as I could determine, was free from deposit of any kind, whilst the dark central part seemed made up of a fine net-work of veins. However, my colleague, Dr. Little (than whom it would be difficult to get a more competent opinion on an histological point of the kind) assures me he could find no adventitious deposit, or structural disease of any kind.

He says:—"The organs are of very small size, though not perhaps

smaller than might be expected in a woman of the advanced age of the patient; this smallness in size will be observed to have occurred almost wholly at the expense of the medullary portion. They presented an universally pale aspect—a characteristic, however, shared by all the other viscera. The capsules are occupied in no part by any local morbid development. The microscopic sections have the natural distinctions of the cortical and medullary regions well and normally marked. There is considerable deficiency (as compared with the healthy adult organ) of the latter."

I regret the semi-lunar ganglia were not examined microscopically.

Dr. Thomas Little has kindly given me the details of a case of bronzed skin, which occurred in the practice of Dr. Lynn, of Sligo, and which he (Dr. Little) had assisted in the examination of. In this case too the supra-renal capsules were found perfectly healthy in structure; the organs were unquestionably of small size, the diminutive size being more remarkable, as the lady was not more than forty-five years of age. This lady, for some time before death, was of unsound mind. Though in sufficiently affluent circumstances, she was perpetually haunted by a fear of poverty, so much so as to deny herself the necessaries of life. This patient had been severely burnt about the hands and face from a gas explosion, and the scars left were the parts on which the discolouration became most strongly marked. Trousseau[a] makes a similar observation. The bronzing, however, was well marked in other parts of Dr. Lynn's case, especially over the legs and arm-pits. Dr. Lynn writes of the case thus—"I have no hesitation in saying that there could not have been a better marked case of melasma than that of Mrs. C.; still there was no supra-renal disease."

I will now briefly call your attention to some cases of bronzed skin, which have been comparatively recently brought before the Pathological Society, and in what respects my case may be said clinically and pathologically to differ from them.

Dr. Hayden,[b] in 1865, showed the pathology of a case in which asthenia, vomiting, and the main symptoms were analogous to mine, but the discolouration in his case was more in blotches; in this case both supra-renal capsules were much enlarged, hard, and nodulated, and in another case which Dr. Hayden subsequently brought before

[a] Vol. iii., p. 536.

[b] Report of the Pathological Society, Dublin, Vol. ii., Part iii., and Vol. iii., Part i.

the Society, the discolouration was in patches over the neck, chest, and abdomen, with intervening white skin, and in this case the cortex of the left supra-renal capsule was occupied by a more solid yellowish matter, which was found to possess all the characters of tubercle, and tubercles existed in the right lung.

In Dr. John Hughes'[a] case, whilst there was slight general discolouration, in some parts the "bronzing" was well marked, especially on the abdomen. Miliary tubercles were found over the pleuræ, and the supra-renal capsules were increased in size, and contained a deposit of lardaceous, yellowish, white substance, like a cut parsnip, which, on a microscopic examination, was manifestly of a strumous nature.

Dr. Heslop, of Birmingham, has published a case (in the *Lancet*, June 4, 1870), in which the deepest tint of "bronzing" was on the penis, except one spot, the cicatrix of a chancre, which remained perfectly white; and the late Professor Trousseau found a "bronzed" appearance of the face, and a blackish colour of the penis in a case of saccharine diabetes, and no morbid state of the supra-renal capsules.

Dr. Habershon's[b] typical case of "bronzed" skin, with disease of the supra-renal capsules (with which we are familiar through the Sydenham Society), if I am to judge from the picture, was not nearly so dark a case as mine. He accounts for the progressive character of the disease by the extensive deposit in the supra-renal capsule, keeping up a persistent irritation and exhaustion of the vaso-motor nerve. He believes the vomiting is due to the irritation of the branches of the pneumogastric nerve supplied to the gland, and the excessive prostration to the extension of the disease to the semi-lunar ganglion. In my case no such supra-renal deposit existed, and yet the symptoms of asthenia, vomiting, dementia, and "bronzing" of the skin, were progressive and persistent.

Dr. Addison said if he saw a patient who presented this peculiar discolouration of the skin, he offered no explanation of as to how, why, or where it came. He observed associated with that discolouration a certain train and combination of general symptoms, a pearly eye, a feeble pulse, a disposition of strongly marked anæmia, and a few other symptoms less constant and less urgent, and he then said, "there is a case in which you will find disorganization of the supra-renal capsules, the body is examined, and no other

[a] Dub. Quart. Journal, Nov., 1865.

[b] Guy's Hospital Reports, Vol. x. 3rd Series.

organs are found diseased,"[a] and Dr. Wilks considers that what is more remarkable than the phenomena of the disease is the fact, that any other conclusion than Addison's can be arrived at concerning it; and further, he says, in most cases published "to disprove the connexion between 'bronzing' of the skin and disease of the supra-renal capsules, the pigmentation has occurred in patches, and in others there can be little doubt that jaundice, pityriasis, ephelis, and ichthyosis have been mistaken for true discolouration." Now, every salient symptom alluded to by Addison was present in my case, whilst the "bronzing" of the skin did not occur in patches, and I can safely say it could not be mistaken for jaundice, pityriasis, ephelis, or ichthyosis; on the contrary, it was general, and to such an extent as to exceed any case I ever saw or read of, and yet there was no evidence of supra-renal disease. Viewing the case fairly, as far as our pathological and histological knowledge goes, it must be called a case of tuberculosis, but with no supra-renal disorganization.

ART. VII.—*Case of Aneurism of the Abdominal Aorta in a Female.* By ARTHUR WYNNE FOOT, M.D., T.C.D.; Fellow of the King and Queen's College of Physicians.

FROM the admitted rarity of aneurism of the abdominal aorta in the female, the following case appears to be of sufficient interest and importance to justify its being put on record. The subject of it was a married woman, without any family, thirty-five years of age, who sought medical advice on the 30th of July, 1870, on account of dyspnœa, palpitations of the heart, and sleeplessness. No reference was made to the abdomen, as the seat of any annoyance, or even of any uncomfortable sensation. The action of the heart was very irregular, a double murmur was audible both at the base and at the apex, and the area of cardiac dulness was of more than twice the normal extent. Two years previous to the present time she had an attack of hemiplegia, affecting the left side of the body, since when her health had not been good; the muscles of the affected side had remained paralysed, but unwasted. This woman continued under observation from the 30th of July till the 8th of the following month, on which day she died. During the greater part of this time the dyspnœa was continuous and extreme, preventing her from

[a] Guy's Hospital Reports, Vol. viii. 3rd Series.

lying down; the palpitations were severe, but intermittent, and all her distress arose from want of sleep, and from symptoms referable to disease of the heart. The abdomen was more than once examined for the purpose of ascertaining if there were any accumulations in the bowels, and on these occasions no tumour was detected, nor was any pain or tenderness complained of during the manipulation of the belly; at no time did she suffer from pain in the back. On the night of the 6th of August she became suddenly affected with a dull aching pain predominating in the left iliac region, and accompanied with a sharp pain, "like cramps," down along the left lower extremity; on the following morning (7th) the pains continued, the left iliac, lumbar, and hypochondriac regions were occupied by a tumour which was dull on percussion, and gave to the hand a hard lumpy sensation, as if the descending colon were extensively obstructed with hardened fæces; the pulse was under 100, weak and regular, the dyspnœa had moderated, and she was able to lie flat, without any distress, and although her countenance was altered, it did not wear a pinched or anxious expression. Poultices were applied to the abdomen, and an enema administered, which, however, did not remove any fœcal matter from the large intestine. The following morning (August 8th) there was no alteration in the position or feel of the abdominal tumour; there was no tenderness over any part of the belly, the face was very pale, the pulse weak, respiration very feeble in both lungs, particularly in the left; she sank gradually, and died at 2 p.m. on this day.

The case was one sufficiently puzzling to cause every effort to be made to obtain an opportunity of inspecting the interior of the abdomen; the rarity of the disease, the absence of abdominal symptoms, and the want of violence in the symptoms which accompanied the suddenly appearing tumour, were not calculated to suggest what turned out to be the case, that an aneurism of the abdominal aorta had become ruptured, and given rise to an extensive retro-peritoneal extravasation of blood.

The body, which presented a very much blanched appearance, was examined twenty-four hours after death; the tumour felt as it had done during life, and had not subsided after death, as many abdominal tumours so often do. When the belly was incised longitudinally, a large black mass presented itself in the situation of the tumour observed during life; in magnitude it might be estimated as representing about three pints of fluid blood; it was situated in the connective tissue behind the peritoneum, and had pushed the

omentum to the right side; the descending colon, relaxed and flattened, was moved forwards, and almost as far towards the right side as the middle line of the body. There were no appearances of peritonitis, recent or chronic, neither adhesions, nor any effusion of serum, pus, or lymph. A true aneurism of the abdominal aorta was found on the vessel immediately above its bifurcation, about the size of a full-sized lemon, but more like a pear in shape, as the broader part of the sac was inferior; the effusion of blood had taken place from an opening at the left side of the tumour, immediately above the left common iliac artery; the opening was situated at one of the most saccular portions of the dilated vessel, and presented the appearance of a squarish aperture, having a fringed margin; the mouth of the opening was obstructed by coagula, some of which were evidently of recent formation from their colour, and the remainder were portions of the stratified fibrine, which elsewhere lined the sac. The common iliac arteries were not directly implicated in the aneurismal sac, but the left vessel was completely obstructed at its origin by the fibrinous lining of the sac, which had become, as it were, loosened from the inner wall of the aneurism, and had shifted its place, so as to slide over the mouth of this artery, so that a forceps passed up the left common iliac artery, did not enter the sac, because it was stopped when just about to do so by a dense leather-like layer of fibrin, while there was no narrowing or obstruction of any other part of this artery. The laminated coagula lining the aneurism were, at the place which corresponded to the rent in the tunics of the sac, quite softened and destroyed, presenting an aperture corresponding in shape and size, as well as in situation, to that which gave exit to the blood; the sudden escape through this rent may have so dislodged and shifted the inner shell of the sac, as to have closed over the mouth of the left common iliac artery, as there were no symptoms during life, and no pathological appearances in the vessel to indicate its having been occluded prior to the rupture of the aneurism. The length of the anterior surface of the sac, measured vertically, was $4\frac{7}{8}$ inches; its circumference at its widest part was $7\frac{1}{4}$ inches; the artery above the aneurism was extensively atheromatous; there was no destruction of the vertebræ or prævertebral structures, but the left psoas muscle was so much softened and disintegrated as to admit of its removal with the fingers. The pericardium contained several ounces of reddish serum; the heart was enlarged; the aortic valves insufficient; the cavities of the right side dilated; the four chambers contained soft

black coagula; the arch of the aorta was dilated, atheromatous, stiff, and brittle.

Before alluding to the rare cases of aneurism of the abdominal aorta in the female, it may be well to consider the relative frequency of aneurism of this vessel, entirely irrespective of sex, as, on doing so, it will be found that, in liability to this disease, the abdominal aorta, even in males, ranks far behind the popliteal, and very far behind the thoracic aorta. The most reliable tables of aneurisms of the spontaneous or non-traumatic variety are those of Crisp,[a] a proof of their reliability is their quotation by Lebert, in his great work,[b] even in preference to the more recent and more extensive ones of Broca.[c] In Crisp's table of 551 spontaneous aneurisms in both sexes, selected indiscriminately from the British medical and surgical journals, from the year 1785 to the year 1847, there are of the thoracic aorta 175 cases, of the popliteal artery 137, of the abdominal aorta and its branches 59. Those who put little faith in statistics on such matters, may read the forcible remarks on this subject of Sir W. Jenner, in his Clinical Lectures on the Diagnosis of Extra-pelvic Tumours of the Abdomen.[d] "Remember that, although by no means to be reckoned among what are called rare diseases, aneurism of the abdominal aorta is not at all common; that you will meet with a thousand cases, I was going to say, of strongly beating tumour in the abdomen, before you meet with one case of circumscribed dilatation of the aorta; so, instead of being your first, it should be your last idea, that an abdominal pulsation is due to aneurism." These observations are well calculated to enhance the value, if such were possible, of the seven cases of aneurism of the abdominal aorta in the male, given with all the richness of accurate detail by Dr. Stokes, in his work on *Diseases of the Heart and Aorta.* Of Crisp's 59 cases of aneurism of this vessel, 51 were males and 8 females. After a somewhat extensive research, confined exclusively to British works, I have been able to find the records of seven additional cases of this disease in the female, published subsequent to the appearance of Crisp's Prize Essay. In more than one of these there was the same entire absence of symptoms referable to the cavity in which the disease existed, that was noticed in the case which suggested these remarks; and in some, death resulted from totally different causes.

[a] Diseases of the Blood Vessels. 1847. P. 113.

[b] Traité d'Anatomie Pathologique, 2 vols., folio. 1857. Tome 1, p. 520.

[c] Des Anévrysmes. Paris, 1856.

[d] British Medical Journal, 16th Jan., 1869, p. 42.

Mr. Porter[a] laid before the Pathological Society of Dublin, 31st January, 1857, a specimen of abdominal aneurism which occurred in a female, who dropped dead as she was walking through Dame-street. She was a prostitute, thirty years of age, and had been continually drinking for the previous three or four years. About twelve months before her death she had taken a course of mercury, and up to the previous July had enjoyed very good health. About this time she began to suffer from a pain in the abdomen, which she ascribed to flatulency, and took little notice of. One night as she was walking through Dame-street, her knees gave way under her and she fell to the ground. On opening the abdomen and removing the intestines, a large aneurismal sac was found engaging the abdominal aorta, about half an inch below where the cœliac axis is given off; it had given way at its posterior and inferior part, and a large quantity of coagulated blood was poured out behind the peritoneum. Mr. W. F. Barlow laid before the Pathological Society of London,[b] 16th March, 1852, an aneurism of the abdominal aorta, taken from the body of a woman, aged twenty-nine, pale, weakly, and of irregular habits, who died in the Westminster Hospital from the effects of a blow upon the head. There appeared to have been no symptoms of the aneurism during life; it was as large as a lemon, and not unlike one in general shape, and was situated opposite the last dorsal and first lumbar vertebræ, but had not, apparently, caused much absorption of those bones. It seemed formed entirely at the expense of the posterior wall of the vessel, the anterior appearing healthy; the cœliac axis and superior mesenteric artery were seen arising from the latter, and were not in the least obstructed. So completely full of coagulum was the sac, that the blood latterly must have flowed onwards much, as though no aneurism had existed. Dr. Keiller, President of the Edinburgh Obstetrical Society, brought before that Society,[c] 27th June, 1860, an important case of this affection, in which an abdominal tumour had been observed during life, and had given rise to considerable differences of opinion as to diagnosis. The patient was an unmarried washerwoman, aged forty-four, admitted into hospital for a pain in her right thigh, and a swelling of the abdomen. She lay on her back with the right limb bent nearly to a right angle at the knee—this position, on account of the weight of the tumour giving her the greatest ease.

[a] Dublin Hospital Gazette, 1857. Vol. iv., p. 92.
[b] Reports of Path. Soc. London, Vol. iii., p. 310.
[c] Edinb. Med. Jour. Vol. vii., p. 495.

Above Poupart's ligament, on the right side, was a large dense hard mass, reaching up to within one inch of the edge of the seventh rib, dull on percussion, but separated from the liver by an interval of clear percussion. The surface of the tumour was tolerably smooth and even; it was quite immovable, and seemed attached to the walls of the abdomen round; a distinct impulse was given to it from the aorta, and a vascular bruit was heard in it; there was no tenderness of the abdomen. She died from exhaustion. The tumour was found to have been caused by a large false aneurism; it contained very little fluid blood, but its interior was occupied by firm fibrinous dots, which weighed about five pounds. It arose from the abdominal aorta, a little above its bifurcation; the opening by which it communicated with the vessel was on the right side, and admitted five fingers. The false aneurism (secondary sac) was formed anteriorly by the psoas and iliacus muscles, and by the peritoneum, posteriorly it was formed by the ilium. There was great erosion of the right ilium, Poupart's ligament was destroyed, and the right hip-joint was opened into. There was no great degree of atheroma of the aorta, and the other organs were natural. Dr. Haldane exhibited to the Medico-Chirurgical Society of Edinburgh,[a] 3rd December, 1862, the abdominal aorta of a woman, fifty years of age, with three sacculated aneurisms springing from its anterior wall, within the space of about three inches. She was also the subject of an aneurism of the transverse portion of the arch, the size of a small apple, and was likewise affected with disease of the aortic valves, in an aggravated form. The symptoms during life, while she was under observation in the Royal Infirmary, had been wholly thoracic, and their causes had been diagnosed to be such as were found in that cavity after death. In Dr. Bennett's work, on the *Principles and Practice of Medicine*,[b] will be found the case of a woman, aged fifty, who presented a pulsating tumour in the pyloric region, attended with the symptoms of organic disease of the stomach; an aneurism of rounded form, nearly two inches in diameter, and almost filled with firm layers of fibrin, arose from the anterior part of the abdominal aorta, at the root of the cœliac axis and superior mesenteric artery, while at the lower part of the thoracic aorta was an aneurismal dilatation, about the size of a walnut, which projected to the left of the vertebral column, directly over the thoracic duct, which vessel it appeared to compress. The

[a] Ed. Med. Jour. Vol. viii., p. 669.
[b] 4th Edit. 1865, p. 618.

study of this very complicated case is likely to prove of much use in the investigation of pulsating tumours in the epigastrium; the progressive and extreme emaciation, and the persistent vomiting, symptoms so well calculated to banish the thought of aneurism, especially in a female, from the mind, were found to have been due to an ulcer of the stomach, and to the propinquity of the thoracic aneurism to the thoracic duct. Another case of aneurism of the abdominal aorta, which occurred in a female, aged sixty-five, whose body was dissected in the Practical Anatomy Rooms, at the University of Edinburgh, has been carefully described by Dr. John Chiene;[a] the abdominal aorta was found extensively diseased, a large pear-shaped aneurism being connected with its lower half. Two very strange cases of abdominal pulsation in females, only one however of which was proved by *post-mortem* examination to have been an aneurism, are quoted on the authority of Mr. Rendle, Surgeon to the Brixton Prison, in the *Essay on Aneurism in the System of Surgery*.[b] A strong, healthy woman, five feet eight inches high, who had never before been in prison, received a sentence of transportation for life. *Immediately* on receiving sentence, and whilst standing at the dock, she almost fainted from terror; was suddenly seized with severe pain in the epigastric region, and fell into the arms of an attendant. The pain in the epigastric region never entirely ceased, and just six weeks after, a pulsating tumour of small size was readily to be felt; this increased in size, until it was as large as the closed fist. She lived for three or four years, being constantly kept in bed and under treatment, and then passed from under Mr. Rendle's care. The second case was proved by *post-mortem* examination to be an aneurism of the abdominal aorta at the cœliac axis, and came on after receiving a severe sentence, though it could not be traced so distinctly to the sentence as in the first case. When the tumour was discovered, it was about the size of a small orange; the woman only complained of obscure pain in the part, and was at her work when first seen. She was at once sent to the infirmary, and put to bed. Twelve hours after being in bed, the aneurism ruptured, and she died in forty-eight hours. A *post-mortem* examination of the body was made. Both patients were women between thirty and forty years of age, and otherwise healthy.

The seven cases of aneurism of the abdominal aorta in females,

[a] Brit. Med. Journal, 22nd August, 1868, p. 201.
[b] 1st Edit. 1862, Vol. iii., p. 351.

just alluded to, and the one which I have given, added to the eight collected by Crisp, make a total of sixteen cases recorded in British works, since the year 1785, and show that, although rare, this disease in women is not so uncommon as to be at once put out of court, in making the diagnosis of a pulsating tumour in the female abdomen. My research in the medical publications of this country since 1847 has been of necessity so incomplete, that I am persuaded of the existence of many more than the seven cases which I have been able to meet with. In answer to the question of why is this disease so much rarer in women than in men, when carotid aneurism is equally frequent in both sexes, and dissecting aneurism more common in females than in males, it is usually said that the alleged predisposing causes of aneurism, syphilis and intemperance, which lead to the precursory disease of the arterial wall, do not operate so constantly in females as in males, and that from the nature of their habits and occupations, females are less obnoxious than males to the exciting causes of aneurism, such as violent muscular exertion, blows, strains, and injuries of various kinds. Some think that the influence of the periodical loss of blood during menstruation on the vascular system of the female, has not been sufficiently considered in reference to the less frequent occurrence of aneurism in women than in the opposite sex. Admitting that the arteries are equally liable to disease of their coats in either sex, it is difficult to avoid concluding that it is the nature of their habits and occupations which render females less liable to spontaneous aneurisms. Thus while there is no reason to believe the popliteal arteries of women, at all events in the lower classes, are less liable to disease of their coats—aneurism of these vessels is so rare in females, that Sir A. Cooper saw but eight cases in his practice of forty years,[a] and on the other hand, in the carotid arteries, aneurism is equally common in both sexes, because these vessels are exposed to a very similar amount of flexion and extension. The predisposition of females to dissecting aneurism may, perhaps, be attributed to the greater delicacy of a less tough connexion between the arterial tunics. With reference to the remarkable latency of symptoms, which has been observed in several of the cases alluded to above, so much so that the *post-mortem* examination on several occasions revealed what had been quite unsuspected during life; it may be noticed that the female abdomen is wonderfully prone to keep secret the existence of conditions

[a] Lectures on the Principles and Practice of Surgery. Edited by Lee. 1837, Vol. i., p. 208.

which might be at once discovered in the male: for this peculiarity there are anatomical and physiological reasons which are obvious and unquestionable, and which account for the wonderful tolerance on the part of females of a degree of ascites, which would in men produce much mechanical interference with respiration, the influence of sex in the case of movable kidneys, and the immense collections of fæces and urine which females can carry about, if not unconsciously, at least with much less inconvenience than the opposite sex are usually able to boast of. Appended to these remarks are some references on the subject of aneurism of the abdominal aorta in females, which may serve as a nucleus for a larger collection of cases. The references refer simply to aneurisms of the main vessel of the abdomen, and not to those of its branches, either primary or secondary; the first eight have been taken from Crisp's table, and all except Nos. 1 and 8 have been carefully verified:—

(1.) Ward, London Medical Repository, Vol. xx.
(2.) Henderson, Edinb. Monthly Jour. of Medical Science, Vol. iii., p. 613, 1843.
(3.) Green, Dubl. Medical Jour., Vol. xxiv., p. 293.
(4.) Shekleton, Dubl. Hospital Reports, Vol. iii., p. 232.
(5.) Shekleton, Dubl. Hospital Reports, Vol. iii., p. 234.
(6.) Hue, Medical Gazette, Vol. ii., p. 317, 1828.
(7.) Burkitt, Guy's Hosp. Reports, 1844, p. 504.
(8.) Hawkins, Medical Times, 1845.
(9.) Porter, Dubl. Hosp. Gazette, 15th March, 1857, p. 92.
(10.) Bennett, Principles and Practice of Medicine, 4th ed., p. 618.
(11.) Barlow, Trans. Path. Soc., London, Vol. iii., p. 310.
(12.) Keiller, Ed. Med. Jour., Vol. vii., p. 495.
(13.) Haldane, Ed. Med. Jour., Vol. viii., p. 669.
(14.) Chiene, Brit. Med. Jour. 22nd August, 1868, p. 201.
(15.) H. Coote, Syst. Surg., 1st ed. Vol. iii., p. 351.
(16.) Foot, Dubl. Quart. Jour., Feb., 1871.

ART. VIII.—*Diseases of the Lungs affecting those who work in Dusty Atmospheres.* By J. ALEXANDER ROSS, M.D.; Resident Physian, North Staffordshire Infirmary.

THOUGH much has been done towards the elucidation of these diseases, by Drs. Greenhow, Peacock, Parsons, &c., in this country, and by Zenker, Schirmer, and others, on the Continent, yet the clinical history of additional cases, presenting as they do different phases, may not be devoid of interest.

I intend to confine my attention to the pulmonary diseases prevalent amongst a class of the above operatives, known as potters,

though the diseases affecting all those who work in dusty atmospheres are very kindred, for, as Dr. Greenhow observes, "the nature of the substance inhaled appears to be of secondary consequence as regards the ultimate result excepting that the heavier and more penetrating kinds of dust, such as angular particles of grit, more speedily excite serious disease than the lighter kinds."

Etiology.—To ascribe to these diseases their direct causes is not a difficult task; these are the inhalation of mechanical and chemical irritants, an over-heated atmosphere, and a deficient supply of oxygen; but to assign reasons why (the direct causes being the same) in one person "chronic catarrhal pneumonia" is developed, and in another fibroid disease of the lungs is much more difficult, the doctrine of "predisposition" or "vulnerability" must here come to our aid; this is well illustrated by the following cases—and first, I will narrate that of a man in whose family no trace of consumptive tendencies could be found; it is an interesting and instructive case, illustrating that those of the family not engaged at dusty occupations remain free from pulmonary diseases, whilst those engaged at such are liable to pulmonary affections of a very chronic character, yet at last bringing the subject of them to a premature grave.

Case I.—H. M., a flatware presser, aged thirty-seven, married, was admitted on July the 2nd, 1870.

His father was a saucer maker (flatware presser), and died at the age of forty-four, from "potters' asthma." His brother is a hollowware presser, and is much affected with asthma; he is obliged to remain at home half time; he is only seventeen months older than this patient.

One sister, four years younger, who works with a "thrower," she is termed a "baller;" has been ill with dyspnœa and bronchitis several times. Three other sisters were paintresses for some years, but they have not been working at it for several years; they are in good health, nor was their breathing ever affected. Painting is not a dusty occupation.

H. M. commenced to work as a potter when eight years old, and was then a mould runner; at this he continued until he became fourteen; he was then apprenticed to a plate maker, at which employment he has continued ever since, except in 1861, when he worked as a labourer for twelve months, trade being slack. For two years past he has complained of dyspnœa, but last winter was

the first during which he was obliged to remain at home; he has always been worse in winter than in summer.

Pains shoot from the upper part of the left side of chest into the right; the sputum contains black spots in a frothy clear fluid.

The right side of the chest in front is pretty resonant; the left side dull, especially over the apex, where the breathing partakes slightly of the bronchial character, but is faintly heard; it becomes more audible towards the base; both lungs are resonant towards the base; both sides are comparatively dull at back in the upper thirds, but resonant at the bases.

There is a harshness with the breathing on both sides of back.

The cardiac sounds are weak but otherwise normal. His breathing is very short and fails him if he exerts himself.

He has a severe cough.

The thermometrical registrations were as follows:—

	Morning	Evening
July 27	99·5	98
,, 28	99	97·6
,, 29	98·4	99
,, 30	98·4	98·6
,, 31	99	97·6
August 1	99·4	98
,, 2	98·6	98
,, 3	99.2	98·6
,, 4	98·6	97·8
,, 5	98·6	98·2
,, 6	99	98·6
,, 7	98·4	98·8
,, 8	99	98·8
,, 9	98·4	99
,, 10	99	98·6
,, 11	98·8	98
,, 12	97·6	98
,, 13	98	98
,, 14	97	98
,, 15	98·2	98·4
,, 16	98·6	98
,, 17	98	97·6
,, 18	97·8	98
,, 19	98·6	98·2
,, 20	99·2	98

The treatment adopted consisted of stimulating expectorants, tonics, and anodynes to relieve the cough.

On the 30th August, he went out very much improved.

The following case demonstrates that, where there are tendencies to pulmonary diseases—the "Phthisical Habitus"—instead of the fibroid degeneration and many years of asthma and winter cough resulting, the *Chronic Catarrhal Pneumonia* of Niemeyer is developed:—

Case II.—T. E., a flatware presser, aged thirty-four, married, was admitted on the 2nd June, 1870.

His father died at the age of sixty-six, and his mother, when much younger, of consumption.

When nine years of age he commenced to work at the pottery trade, being apprenticed to a flatware presser.

He continued at this until he reached the age of nineteen, when he joined the army, in which he remained nine years.

He then recommenced his former work, and has continued at it. For the last four or five years he has had cough in winter; it was present also to a slight extent in summer. During the last twelve months it has become very much worse. He has been temperate in his living.

He is pale, but not very much emaciated. His breathing is very short.

There is dulness on both sides of chest in the subclavicular region, but more especially on the right; here also can be heard large, dry ronchi; the breathing is not so audible on this side. The middle and lower portions of the lungs are more resonant, especially the left. On the right side crepitus can be heard at the middle and lower portions, diminishing in amount from above downwards.

The cardiac sounds are weak. He never has night sweats; skin dry, tongue white and furred, cough troublesome, sputum purulent, and at times blackish.

He complained of griping pains over the bowels, especially over the lower part of the abdomen. He always suffered more or less from diarrhœa, but towards the last it set in with more than usual severity, and was with difficulty restrained by means of sulphate of copper and opium.

He seemed to improve for some time after his admission, but during the last week he sank rapidly, and died on July 4, 1870. He had worked up to the week previous to his admission.

Copious perspirations took place during the last two days.

The autopsy was made a few hours after death.

The body was not very much wasted; the heart appeared to be normal; both lungs were adherent to the chest walls, especially the right. On cutting into the lungs I found the left to be studded throughout with black pigment, and here and there, especially at the upper part, small cheesy masses, none so large as a pea; the apex was converted into a dense, fibrous tissue, for the most part slate-coloured, but containing in its centre a small piece of equally dense, white fibrous tissue; no cavities could be detected; the rest of the lung, especially the lower part, was emphysematous.

The apex of the right lung contained a large fibrous mass, whose centre was excavated, the cavity being about the size of a filbert; the cheesy masses were softened in very many places, and broken down in several. This lung was very emphysematous at its lower parts. I could not detect any true tubercle. When passing the finger over the cut surfaces of both lungs they felt as if studded over with rough nodules.

The liver was of the normal size; in its right lobe there was the cavity of a large abscess which had burst, previous to death, into the abdominal cavity; a small mass of yellow, cheesy matter was situated in the left lobe; it had commenced to soften in its centre. The liver did not present any traces of inflammation, nor had it contracted any adhesions, except in the immediate vicinity of the abscess, where there were some slight ones.

The intestine, from the ileo-colic valve to the rectum was studded with ulcers—transverse ulcers taking the direction of the valvular conniventes, with ragged, irregular edges; near the rectum and in it were some very large ones, as large as a florin, very irregular in outline and of considerable depth, the bowel in this situation being much thickened.

I injected a portion of the lung with Prussian blue, and found, on examining thin sections under the microscope, that the hard nodules were composed of fibrous tissue, and contained a large quantity of black amorphous pigment and whitish particles which did not polarize light. The air cells were completely obliterated; their original boundary, however, was still discernible, being mapped out by fibres resembling elastic tissue fibres, and being almost free from pigment.

The capillary blood vessels ramified abundantly around the nodules, but only one or two very attenuated ones could be

discovered in the smaller nodules, and I did not succeed in detecting any towards the centre of the larger masses.

On taking a section from the outer portion, with part of the vesicular lung tissue attached (which I was able to do, having first embedded a piece of lung in wax), I found that the vessels had become smaller as they entered the indurated portion, and finally were obliterated.

Dr. Parsons, who paid much attention to this subject, is of opinion that the pulmonary affections are confined to flatware and hollow-ware pressers, but though they are especially obnoxious to them, yet instances of the disease affecting "handlers," "throwers," "turners," &c., have come under my notice.

It would seem, indeed, that this affection is not confined to those who work amid dust, for Dr. H. G. Sutton (*Med. Chir. Transacts.* Vol. xlviii.) has given a very clear account of the disease as affecting others. To the beautiful plates accompanying his paper, as well as to those so faithfully illustrating Dr. Greenhow's papers in the Pathological Transactions, I beg to refer the reader.

Pathology.—It will be expedient to divide these affections into three stages.

The first two are very similar in the two classes of cases; it is generally in the third stage that they branch off and run different courses.

The stages are these:—

1. Irritation of the bronchial mucous membrane, followed by bronchitis.
2. Pneumonia.
3. (*a.*) Fibroid Degeneration, or
   (*b.*) Pneumonic Phthisis.

I. *Irritation of the bronchial mucous membranes; Bronchitis.*

These stand in the relation of cause and effect. Bronchitis is the evident consequence of the inhalation of mechanical and chemical irritants, for, in addition to the dust, the stoves in the pressers' ovens generate carboniferous and sulphurous compounds, and consume the oxygen; these irritants cannot long exert their influence with impunity; repeated attacks of bronchitis, generally of a sub-acute or chronic nature comes on, and exudation takes place. In those with good constitutions respite from work and suitable treatment improves the condition very much; the lymph may become in whole or in part absorbed, but the patient, though knowing the consequences, and yet scarce well, resumes his work, other attacks

come on, each leaving its effects behind, and sooner or later the inflammatory products extend to the surrounding pulmonary tissue, thus forming the stage of

II. *Pneumonia.*

In this stage inflammatory exudation into the pulmonary tissue takes place; the subject of it either remains at his work, probably seeking medical aid at the same time, or he may desist from work until the more acute symptoms, if there be any, pass off.

After a time this chronic or sub-acute pneumonia assumes one or other of the characters which introduce the next stage, *i.e.*,

III. (*a.*) *Pneumonic Phthisis*, or

(*b.*) *Fibroid Degeneration*,

according to the constitution of the patient; for now those who possess that "vulnerability" of which Niemeyer speaks, will gradually have developed the phthisical symptoms, which, though latent at first, now, through those influences which "hinder or disturb the normal development and maintenance of the organism," they become manifest; "the inflammatory nutritive changes occurring in them, lead, as a rule, to a very abundant production of indifferent and perishable cells." Suppuration takes place, cavities form, and the illness runs a comparatively rapid course.

But in another class of cases—those that do not possess this vulnerability—the inflammatory exudation is of the plastic type; a connective tissue is formed; this grows and increases until it completely obliterates the air cells, invades the pulmonary tissue, indurates, and eventually cuts off the vascular supply. Inflammation again sets in, this time, perhaps, of a low gangrenous type, and the nodule separates *en masse* from the surrounding tissue, or the nutritive supply being deficient, fatty or cheesy degeneration takes place in the centre, and so it gradually breaks down.

Another train of events sometimes forms the last phase of the illness; the lungs are already very much occupied by these masses, thus seriously impairing their efficiency; but this is not all, the residuary tissue is for most part emphysematous, the heart becomes secondarily affected, for the blood is thrown back on the right ventricle, then through the whole systemic circulation; the retarding process extends to the left side of the heart, eccentric hypertrophy takes place; this state of things obtaining, a slight increase of the pulmonary symptoms gives the already weak and incapacitated heart more work than it can perform; congestions take place everywhere; the lungs become engorged; the already defective

oxidizing process is rendered still more defective as the livid body too plainly tells; the heart at last ceases; and with it the patient's sufferings too.

The following cases will serve to illustrate the foregoing remarks. I will give them briefly:—

Case III.—J. M., aged forty, single, was admitted on the 19th February, 1870.

She was employed in the glost warehouse. There is no family history of consumption. She has been frequently in hospital when suffering from unusually severe attacks of asthma. Until recently, crepitus could not be detected as a constant symptom; it was present now and then, but during the last five or six months it has been a pretty constant one; it was principally of the mucous or sub-mucous variety; there never was any indication of the presence of a cavity in either lung. She has lost her voice. She could not bear the application of the laryngoscope.

The chest walls did not expand, except to a very slight extent at the upper part, there were more extensive movements, however, towards the lower portion.

The inspiratory muscles stood out prominently, and acted energetically, and the integument in the space between the two sterno-hyoid muscles was drawn inwards and downwards with each inspiration; the face bore the expression which we generally see in those suffering from severe attacks of asthma; there was not the hectic look, but the pale anxious countenance to the last. There were sometimes exacerbations in the attacks of asthma. The sputum at first was frothy, afterwards purulent, mixed with blackish lumps, but no blood.

She died on May 17th, 1870.

The *post-mortem* was made fifteen hours after death, when the following appearances were found:—

Body not much emaciated; skin very white and clear; no discolouration; heart occupying the normal position; the lungs did not collapse much when the chest was opened; the upper halves of both lungs were adherent to the chest walls, particularly so in the case of the left, whose tissue gave way in the attempt to take it out.

In the upper portions of both lungs were nodules, and these were broken down to about the same extent, but there was not any cavity in any part of the lungs; the broken down tissue resembled

soft marl, of a slate colour, in the left lung; it was separated from the surrounding tissue, and occupied what would otherwise have been a cavity; bands intersected it, running from boundary wall to boundary wall, the remains of the bronchial tubes and blood vessels; these bands retained the *débris* in their meshes.

The right lung was much in the same condition, there was no distinct cavity, but when an incision was made into the lung, this tissue could, with the greatest facility, be extricated from between the tubes, leaving the whole of the upper part of the lung completely excavated; this substance yielded a large quantity of slate-coloured juice; there was not any pus, nor could I detect tubercles in any part of the lungs.

There were many indurated nodules scattered throughout the upper two-thirds of the lungs, but none had undergone cheesy metamorphosis. The other organs were healthy.

Case IV.—W. B., aged forty-four, married, was admitted on April 6, 1870.

This patient was a cup and bowl maker (a branch of flatware pressing).

There was no family history of consumption.

He has had a cough for many years, but during the last two it has become much worse.

He did not expectorate much until six weeks ago, then he says it contained hard black lumps, afterwards it became more purulent.

During the last three years he has complained of an aching pain in the left side of the chest and at the epigastrium, together with palpitation; nevertheless he continued to work up to three months ago, then he commenced to sink rapidly.

For some time previous to his admission he could not sleep when lying down, and he could not lie on his left side.

When admitted he was suffering from dyspnœa; he was pale and somewhat emaciated; he wore an anxious countenance; the expectoration was purulent; there was dulness over the apex of right lung, and about the upper third of the left; there was very loud crepitation; on account of this and the rapidity of the breathing it was impossible to distinguish the heart sounds; the pulse was regular, but very weak; he was unable to lie down, seldom slept, and then only for a short time; there was no dropsy.

He remained much in this condition until next morning, when there was some abatement of the symptoms; he then got out of

bed, and sat on a chair with his head resting on a pillow placed on the table; he was in this position when he fell off the chair and expired.

The autopsy was made next day.

The body was found to be emaciated to some extent, the heart enlarged and its walls thin.

The right lung was slightly adherent to the chest wall on its external surface about its middle, where it was contracted, a depression resulting; the left lung also was slightly adherent in places; each lung filled its respective region.

The upper third of the right lung was occupied by a large hard bluish-black nodule, which rose above the adjacent surface. This nodule when cut yielded a slate-coloured juice; it cut and felt like india-rubber. Numbers of these nodules, varying in size from an egg to a millet seed, were scattered through both lungs. The remaining tissue, particularly at the bases, was emphysematous, in some places having the appearance as if a number of air vesicles had coalesced.

The larger nodules in the left lung were situated in the vicinity of the root. In both lungs the least deposit was to be found in the lower portions. There were no cavities nor any evidence of softened tissue.

In the bronchial tubes there was some purulent matter; the mucous membrane lining these was pinkish, and the longitudinal bands well marked, standing out in relief, and leaving spaces between them.

The two lungs weighed 4 lbs. 2 oz. The sp. gr. of a piece of one of the nodules was 1·1098.

Dr. Purser, of Dublin, kindly examined microscopically a section from one of these lungs, and I was much pleased to find the conclusions at which I had arrived verified by so experienced a microscopist.

Sections of nodules from these lungs presented appearances similar to those already described.

The following case is interesting, showing that the heart becomes secondarily affected:—

Case V.—W. F., aged forty-nine, a flint miller, was admitted on the 12th November, 1870.

He has had the usual symptoms of these pulmonary diseases for many years, and has been several times in the infirmary wards for

them. During the latter few weeks of his life, however, the cardiac symptoms became the most urgent, the sounds became muffled, no impulse could be discerned; there was great oppression over the heart, the lower extremities became dropsical, the anasarca spread upwards; at last cellulitis set in, beginning at the left axilla, and spreading downwards. He died in great agony on December 31, 1870.

The autopsy was made fifteen hours after death; then I found that the lungs contained the same characteristic hard nodules, none broken down or softened, but, in addition, there were formations of a more recent date; these were spots of pneumonia, in the stage of red hepatization, easily broken down, and exuding a sanguineous fluid; the lower parts of the lungs were emphysematous, and they were gorged with blood. The heart was more than double its normal size, its cavities were greatly enlarged, and the walls so attenuated that those of the right ventricle measured about a line and a half in thickness, and those of the left not two lines and a half; the cavities were full of blood.

Pigmentary Colouring.—As to the origin of this it has been proved incontestably by Knauf, Zenker, and Traube, that carbonaceous matter can enter the pulmonary tissue. But there is another point to be considered. Why is the pigmentary colouring in the lungs of potters, flax dressers, and others not much exposed to smoke, almost and often quite as dark as that found in the lungs of miners?

Dr. Greenhow says on this subject—"It seems only reasonable to conclude that these fine particles of carbon find their way much more readily and more abundantly into the lungs of persons already suffering from chronic pulmonary disease;" but the explanations of Drs. Parsons and Makellar seem to me to be more satisfactory, at least as regards those in the potting trade.

Dr. Parsons believes that some of the pigment is due to the constant inhalation of carbonic acid, and asks "may this not be the origin of the 'carbonizing process of the protein substances contained in the blood cells' mentioned by Wedl?"

Dr. Makellar believes that it is due partly to inhaled carbon, and partly to the carbon formed in the body not being completely carried off, owing to the deficient supply of oxygen.

Semeiology.—At first the symptoms are those of bronchitis of a sub-acute character, afterwards becoming chronic. After a time, varying as to duration, the second stage sets in; then there are

increased dyspnœa, often paroxysmal, pains shooting across the chest, the sputum is still frothy, but sometimes tinged red if the symptoms are more acute. These symptoms soon give place to those of the more advanced stage, and now the disease takes a different course according to the constitution of the patient.

In the one case the symptoms become those of pneumonic phthisis, and the disease runs a similar course; in the other case, where there is no "phthisical habitus," we see the following symptoms:—

Increased dyspnœa, the anxious countenance, the strong action of the respiratory muscles, wasting to a slight extent, expectoration sometimes frothy, sometimes muco-purulent, containing a considerable quantity of black material; examined microscopically, it is seen to be composed of various cells—epithelial and nucleated globular, and yellow elastic fibres; these latter are not destroyed by strong acids or alkalies. The voice becomes hoarse; the laryngeal mucous membrane is seen with the laryngoscope to be thickened and red. At length the dyspnœa increases, the breathing becomes gasping, the body pallid or even livid, the extremities cold, the heart's action becomes weaker, and soon all evidence of life ceases.

There are important differences between phthisis and this fibroid disease, and these give assistance in forming a correct diagnosis.

In the fibroid disease, the cough is harsh and severe; the expectoration is copious and frothy, composed principally of mucous, with carbonaceous matter; there is considerable dyspnœa, but not much wasting.

In the later stages we find considerable dulness over the upper parts of the chest, but not always under the clavicle, or if on one side the sub-clavicular region be dull, the other side will often be dull about the junction of the middle with the upper third of the chest; and though there are great dyspnœa and severe cough, yet the marasmus, the diarrhœa, the night sweats, and the hectic of phthisis are absent; nor is the temperature so fluctuating.

Treatment.—For the better consideration of this, I will divide the disease into two stages—

I. Curable.

II. Incurable.

The curable stage will correspond to the first and generally the second pathological stages of the disease, provided that the patient ceases to follow his employment.

The incurable stage will correspond to the third stage of the disease. Although I apply the term incurable to it, I believe that even now in many instances, if the sufferer would renounce his employment, the disease would not progress, and might even improve. Special remedies for the disease, there are none. In the first stages stimulating expectorants—ammonia and senega, or squills, ipecacuan, chloric ether, and ammonia—have been found useful, the ammonia especially so if the expectoration is difficult and the phlegm viscid.

Most beneficial aids to the treatment are to be found in the inhalation of iodine, turpentine, Hoffman's anodyne with steam, or even steam *pur et simple.*

Turpentine or iodine is to be recommended where there is dyspnœa and little irritation. Expectoration is thus promoted with the intention of enabling the mucous membrane to throw off the irritating particles. It is not well to continue these agents too long, as the irritation set up may be injurious. Steam alone may be used when they are discontinued.

Where there is considerable and persistent irritation, with free expectoration, I have found the inhalation of steam, impregnated with Hoffman's anodyne, or a strong solution of bromide of potassium, administered by means of Siegle's steam spray producer, useful.

Where there is very copious and long-continued expectoration, the

Glycerinum Acidi Gallici, or

Glycerinum Acidi Tannici,

(1 part to 3 of water), exhibited with Siegle's apparatus, will be beneficial. It must be used directly after the addition of the water, otherwise the acid will be precipitated.

In the later stages tonics, as ammonia and bark, iron and quinine and cod liver oil, are the remedies. A very effective palliative for the distressing cough will be found in the use of chloral and opium, in combination as follows:—

℞. Chloral hydrat, gr. v.
Vini opii, m. x.
Syr. tolutani, ʒii.
Aquæ ad. ʒi.
M. Sumat ʒss. pro re nata.

The following also is a useful remedy when the cough is accompanied with sickness:—

℞. Liq. morph. acet, ʒi.
Acid hydrocyan (Scheele), m. iv.
Acid nit. dil., m. xx
Spt. chloroformi, m. xx.
Syr. tolutani, ʒiv.
Aquæ ad., ℥i.
M. Sumat., ʒi., p. r. n.

It is the "Linctus morphiæ" of the Pharmacopœia of this Infirmary.

It now becomes my pleasing duty to express my thanks to Drs. Arlidge and Orton, and to Mr. Ashwell, for the full investigation of the cases under their care, which they have freely permitted me to make.

ART. IX.—*On the Views of Niemeyer regarding Phthisis.*[a] By HENRY KENNEDY, A.B., M.B.; one of the Physicians of the Cork-street Hospital, and attached to Sir Patrick Dun's.

THE following remarks have been put together, I must admit, somewhat hurriedly, but I was anxious to elicit a discussion on a point which appears to me one of very great practical importance; and I knew no place more suited for my purpose than the meeting I have now the honour of addressing. It will be known to all present that recently a German work on medicine, by Niemeyer, has appeared; and has already been published in an English garb; so that it has obtained a considerable amount of notice from the profession, and this has been increased by the separate publication of one important portion of the work; viz., *The Clinical Lectures on Pulmonary Consumption.*[b] It is to these lectures I would ask attention; for they appear to me to enunciate views, which, to say the least of them, are open to discussion. It may be observed here that the main point of the author's views consists in the idea that inflammatory action is the basis of all phthisical disease. He does not, however, claim the idea as original; but mentions specially the names of Virchow and Buhl, as holding and teaching similar doctrines; whilst we know that in England several physicians, and more particularly the late Dr. Addison, promulgated like views, and

[a] Read before the Medical Association of the College of Physicians, January 18th, 1871.

[b] Clinical Lectures on Pulmonary Consumption; translated by C. Bæumler, M.D.

published them several years back.[a] It may not be out of place here to quote a few of the expressions found in these lectures, and which embody the views of our author. To many, I suspect, they will seem as strange as they do to myself. The following are examples of what I mean:—"In the present stage of science there is but one kind of tubercle, miliary tubercle; and but one form of tuberculosis, and all those changes, which, since Laënnec, have been designated 'infiltrated pulmonary tubercle,' are the product of chronic, especially of catarrhal pneumonia." Again, "In very many cases there is not a single tubercle found in phthisical lungs." Again, "The tubercles, in the majority of cases, are clearly of recent origin, and have complicated the pulmonary phthisis, when it was already in an advanced stage." And, once more, "He speaks of patients who, after having suffered from phthisis for years, had, at last, become affected with tubercles. He also uses the expression, that the greatest danger to most phthisical patients is the development of tubercles."

From these passages it will be observed that the author, assuming that inflammatory action is the basis of phthisical disease, and declares itself in the form of pneumonia, goes on to state that the deposition of tubercle is quite secondary, and that the views held and taught by Laënnec, must be abandoned. Before going further I must here notice that the title of this, the first lecture, is "The Pathology of Phthisis." Now to these words, and the way they are here applied, I must strongly object; for the author all through argues as if the morbid anatomy were the real pathology of phthisical disease; and nowhere does he, as far as I am aware, allude to that general state of the constitution, which precedes, and, to my mind, must precede the development of the disease. This arises from confounding morbid anatomy with pathology; than which there cannot, to my mind, be a greater mistake; and it seems to pervade the entire views of the author. That a something precedes the pneumonia, to which the author gives such a prominence, in the formation of tubercles, is, I think, absolutely certain. Yet, I repeat, he nowhere expresses himself to this effect; but expressly

[a] Since writing what is stated above, I have read again Dr. Addison's paper; and would now observe that there appears to me a very marked distinction between his views and those of our author; the latter makes inflammatory action, in the shape of pneumonia, the basis of all tuberculous disease; that is as preceding it; while the former sums up by saying, "Inflammation constitutes the great instrument of destruction in every form of phthisis;" a statement bearing a totally different meaning, and from which, I rather think, very few would dissent.

states that pneumonia is, in the vast majority of instances, the starting point of phthisical disease; and, at page 13, he asserts that no one form of pneumonia gives rise to the affection; but that all do so, though not equally. And this leads on to the main question; are these views correct? Is inflammation the starting point of phthisical disease, or of phthisis? Or is pneumonia? My answer to these questions must be in the negative; and I cannot help considering the views advanced by the author to be too confined, and not taking that wide grasp of the subject which it appears to me to require. In truth it is but a theory, as it were, which, I believe, does not square with well ascertained facts. In this respect, and with my present knowledge, I would still hold to the views so long since advanced by Laënnec; and I only wish that either the subject had fallen into abler hands to treat, or that more time could have been devoted to the question by myself.

Here it must be observed that the line of argument I would pursue leads me to state some facts which are patent to all. Thus it will not be denied that the locality where tubercles are, in by far the majority of instances, found, is the upper lobe of the lung; or I might say the upper part of the lobe. And nothing is more common than to find a few scattered tubercles here, of which no sign whatever had existed during life. I call them scattered, for there is no kind of union amongst them, and certainly nothing of induration around them. I repeat that such a state of parts is, in my experience, very common; and often exists without any trace of scrofulous disease in any other part of the frame. In fact the patients die of other and totally different diseases. In keeping too with this statement I appeal to the experience of all present as to the earliest physical signs of tubercular phthisis. It would be out of place here, even were there time, to enter into these. But I would ask do they afford any evidence of an existing pneumonia. Is there any crepitus at this stage, or expectoration? On the contrary, is it not known that the cough is dry, and that the presence of crepitus only makes itself known after months, it may be years, during which the presence of other physical signs existed. Even when it occurs, and I admit it may occur, that expectoration is an early symptom of phthisis, its character is not that of pneumonia, as I think all will agree with me. Neither is the crepitus of early phthisis at all like the crepitus of pneumonia; the one is a single glug, and of large size; the other very fine, and the sounds heard numerous.

But if the progress of tubercle be traced further than this mere deposit in the lung, do we find any more confirmation of the views of our author? I believe not. We do, indeed, find that in certain parts of the lobe they become more numerous, in fact, close to each other, with a kind of gelatinous deposit joining the one to the other, but nothing at all of what could be called pneumonic solidification, and any softening process and formation of cavities occurs subsequently to all this again, and, in the first instance, takes place where the tubercles have been congregated and massed together.[a] It is in this state, as I believe, that the tendency to attacks of intermittent pneumonia are so apt to occur, and, of course, add to the serious character of the disease. The readers of Andral may recollect the cases he has detailed, which bear so directly on this point; and I am sure there are none present who have not met similar cases. I have said that it is when tubercles are grouped closely together they begin to soften, and so form cavities; for I believe it to be very exceptional when single tubercles suppurate. Still this does occur, and I have seen it, and have even known instances where a very few tubercles existed near the surface of the lung, and where one of these had given rise to pneumo-thorax. Speaking of isolated tubercles reminds me too of the state they so often, I might say constantly, present in children. Here, as you all know, they will pervade the lungs, liver, spleen, kidneys, and brain, and yet nothing but tubercles in their most simple and uncomplicated form will be found.[b] This part of my subject might be pursued much farther, but my limits forbid.

The attention of the meeting must now be called, for a few moments, to the pneumonia which our author makes the foundation of all phthisical disease. And here I must ask, Is there anything in its natural history to justify such an idea? My hearers know perfectly well that, as the upper lobe of the lung is the common seat of tubercle, so it is in the lower that pneumonia manifests itself. Here, then, as it appears to me, is a most material point in the question, and one which it seems to me impossible to reconcile with his theory. But further, is the progress of this disease—now so well ascertained—capable of, in

[a] It is, I believe, too generally admitted that the softening process takes place from the centre of the mass, not around it.

[b] In children that die of hydrocephalus, the state of parts described above is very common. The glands too, particularly of the mesentry, will be often found enlarged, but rarely suppurated.

any way, supporting the author's views? I think not. For is it not known that pneumonia is, in the great majority of instances, a disease amenable to treatment, and that it is very exceptional when it becomes chronic, and still more so when it leads, or seems to lead, to the deposit of tubercles? And here I cannot but observe that the author appears to me to make a grave mistake in classing together all the kinds of pneumonia as giving rise to tubercles. He says, indeed, there may be some difference in degree; but this is all he allows on the point. Surely I am not wrong in saying that the experience of all the physicians of this city is diametrically opposed to such statements. For myself, I believe that it is only one form of pneumonia—that known as the strumous—which leads on to the deposit of tubercles; and, on the contrary, that the vast majority of cases lead to no such end.

But it will be asked here, do we not meet with the state of morbid parts so much dwelt on by the author? And how are they to be accounted for? In answer to these questions, I would, in the first place, observe that the author seems to me to take quite too narrow a view of the subject. Thus he speaks of tubercles as if they were a something totally different from the other morbid changes with which they are often found to co-exist. I take it that this is a very erroneous way to look at the question. For to my mind it matters not whether the tubercles be grey, or yellow, or accompanied with cheesy deposits, or indurations. They are all the result of a state of the constitution known as the strumous; nor does the presence of a crop of fresh tubercles—on which the author places so much weight—alter this view. I see no grounds for thinking why strumous disease should be different from other diseases; and as they present certain differences in themselves, and yet are the same diseases, so is it with strumous disease. I think too it is an accepted fact, as taught long since by Laënnec, that strumous disease, whether in the form of tubercles or otherwise, developes itself only at certain periods. Hence, a simple explanation of what is so often found—I mean a crop of recent tubercles supervening on much older standing disease. And this shows itself at times in a very striking way. For it is not by any means uncommon, after strumous disease has declared itself in the chest, for the symptoms to subside somewhat suddenly, but only to be followed by more serious disease—if such were possible—in the abdomen, or more particularly in the brain. A very recent example of this latter occurrence I saw very lately with my

friend, Dr. R. Kirkpatrick; and had I not seen similar instances before, I would surely have been puzzled as to its nature.

I have already stated my conviction that the views of the author are too limited; and, as I read the lectures, it seems to me more and more of this is to be discussed. For what will any one say to the following, which I quote from page 27 of these lectures? —"I consider the almost universal opinion, that pulmonary consumption arises independently of accidental or immediate exciting causes, in consequence alone of a diathesis, to be as unproved as it is dangerous." In other words, there is no such thing as a "diathesis" or pre-disposing cause for the occurrence of phthisis. If this be true, others with myself have been grievously mistaken; but, as I can only speak for myself, I hold that pre-disposition is all in all in the production of phthisis; and further still, that the marks of such a constitution are perfectly well known, and, in these countries at least, long recognized. This decided opinion too, I would just observe, does not in any way clash with the possibility of phthisis declaring itself in an individual otherwise healthy, but who has been long exposed to the causes which are known to lead to the disease.

In keeping with what has been just stated, about there being no strumous diathesis, are the remarks of the author on the occurrence of hæmoptysis, on which he dwells at considerable length, and states that proper views have not been held on this point. For he holds that when it happens the blood acts at once as a foreign body, which causes inflammation, and this again leads on to the deposition of tubercle. The author must have some local cause to support his theory, and so the blood is turned to account. Need I say to this meeting how he has overlooked the state which has preceded the bleeding; for a healthy man will not so bleed, and, as was said before, there must be the pre-disposition.[a]

In the last place, my hearers will not be surprised to learn that the author denies the hereditary nature of phthisis—as I take it one of the best established facts which statistics have ever given us. But this would not suit our author or his theory; and so he has ignored it. It is not easy, however, to understand any one carrying his views farther than this. It reminds me of the Belfast

[a] Bleeding into the lung, as all know, is not, by any means, necessarily followed by tubercular deposit; and when it does occur, in connexion with phthisis, it is much more frequently an advanced than an incipient symptom; at least this has been my own experience. In other words, it supervenes on disease, long previously existing.

physician who, to support his theory, has denied all value to cod-liver oil as an agent in the treatment of phthisis.

Other points than those noticed might easily be found in these lectures which would call for discussion. Thus at page 6 is the following passage:—"The conditions under which the product of a common pneumonia undergoes cheesy degeneration are not known to us; but we must particularly mention the fact that this termination is not only observed in those persons who have tubercles, old cheesy deposits, or cavities in the lungs, but at least as frequently in previously healthy individuals, and especially in those affected with long-standing emphysema of the lungs." There is more than one point which might be noticed in this passage; but I have merely given it because the author speaks of the union of "long-standing emphysema" with strumous disease of the lungs. From my own experience, I must directly question this statement; for I believe the occurrence of emphysema to be quite antagonistic to the presence of tuberculous disease of any form, and *vice versa;* tuberculous disease is, as far as I have seen, as rarely seen joined with emphysema.

A second point which may be noticed here as bearing on the general question is the fact that tubercles cannot be injected. Now, on the theory started by the author, it seems to me very difficult, if not impossible, to account for this. For if inflammation were the starting point, and tubercles the direct result of this inflammation, why should not the injection pass into the latter? Yet it does not do so, as any one may satisfy themselves by looking at some of the specimens of diseased tuberculous lungs to be seen in the Museum of the College of Surgeons. Some of these specimens are very remarkable; for while the whole lung is studded with tubercles presenting their ordinary whitish appearance, all the rest of the organ has freely received and been deeply coloured by the injection.

Though I have no intention here of saying one word about the treatment of phthisis, I cannot help observing that if the author's views were correct, it would truly be a very simple matter. A modification of the antiphlogistic plan, suited to each individual case, would as surely cure it as it does common pneumonia. Nay, it should yield much easier; for the amount of lung involved would, speaking generally, be very much less than when common pneumonia existed. Yet is this the fact, or has the experience of any one present confirmed it? On the contrary, is it not certain that

while pneumonia is, in a very large per centage, amenable to treatment, tubercular disease is the very opposite, and it may even be doubted whether we have any direct control over it at all. Certain it is that it is obstinate to a degree, and I fearlessly assert that no stronger contrast could be afforded between any two diseases, than between common pneumonia and ordinary tuberculous disease. But I cannot pursue this point further here. Before concluding, I would wish to observe that I do not, for one moment, question the several statements advanced by the author in these lectures; and more particularly what he has advanced about the morbid anatomy of scrofulous disease of the lungs (not, let it be observed again, the pathology). I have seen all the states of the lung he describes; but I do directly question the line of argument he takes, and the mode of reasoning he applies to his facts, for I believe the interpretation he puts on them to be erroneous, and impossible to reconcile with well ascertained facts; and here I cannot help observing, and I think those who have read these lectures will agree with me, that they strongly convey the idea that his cases are few and far between, selected from this and that source, while not more than four or five are given altogether. Now I may be mistaken, but I cannot help thinking that this is not the way a great question of the kind should be treated. It is surely not the exceptional cases which are to determine the point, and above all, in such a common disease as tubercular phthisis is known to be. We are to be guided by the hundreds not by the units. Yet it seems to me our author has used exceptional cases only for his argument; and when I recall the facts advanced by myself this evening; when I think of the absence of physical signs, which ought to be present when they are not; when I recollect the different localities which the two diseases he has brought into such close connexion—union I may say—occupy; when I contrast the difference of duration and treatment in the two diseases; when I find that he denies there is such a state as the strumous diathesis, and ignores hereditary transmission as tending to lead to phthisical disease; when, I say, I consider these several points, I can, on my part, arrive at no other conclusion than that the views advanced by Niemeyer cannot be sustained, and that these lectures are not worthy the name of the author.

ART. X.—*The Pneumatic Aspirator.* By ROBERT ST. JOHN MAYNE, L.R.C.S.I., L.K. & Q.C.P. ; Surgeon to the Meath Hospital and County Dublin Infirmary, Lecturer upon Anatomy, Carmichael School of Medicine, &c.

IN the present age of scientific advancement, when so many strange instruments and novel appliances are being constantly submitted to the profession, it is most desirable that the practical surgeon should distinguish between those that are mere curiosities, and those that possess some real merit and decided superiority.

The advantages derived from the exclusion of air in certain operations, and under certain circumstances, were observed by the older surgeons, and are at the present day very generally admitted. In the treatment of chronic abscesses, in the removal of fluids contained by synovial and serous membranes, more particularly the larger joints, and the pleural cavities, this accident is most carefully to be guarded against. With this object in view, many methods of operation have been advocated, and many instruments devised. Abernethy, in the evacuation of chronic abscesses, recommended a valvular incision, steady digital pressure, and the wound, if possible, to be healed by the first intention. Subaqueous incisions, a proceeding always unmanageable, and opening under a stratum of carbolic putty, have likewise their several supporters.

For the purpose of preventing the entrance of air in paracentesis thoracis, South states that various valvular cannulas with stopcocks, and syringes with ball valves, have been employed. Bouvier used a cannula with a ball valve; Reybard a cannula with a bladder; Recamier a trocar, the front aperture of which, by drawing back a stilette, was covered with a spring pad; Schuh, a trocar and cannula, furnished with a stopcock, to which was attached a trough in which the fluid, as it escaped, was collected, and might be raised higher than the opening of the cannula, being guarded with a leather valve; Baum recommended a piece of gold-beaters' skin so placed over the aperture, that only a small opening for the fluid remained, which could be readily closed should the air threaten to enter.

The Pneumatic Aspirator, but recently brought under the notice of the profession, seems to present facilities for the performance of this operation far exceeding any of the methods above mentioned. In the last number of the *Dublin Quarterly Journal*,

Dr. James Cuming, of Belfast, has given an excellent description of the origin and character of this instrument. He says:—"It consists of a strong glass tube, about the size of an ordinary stomach pump syringe, and fitted up in the same manner with two cocks. In this is an air-tight piston, by pulling up which, with the cocks closed, a vacuum can be formed. For the instrument is arranged a series of tubular needles and trocars, beginning at the ordinary size and decreasing to the finest calibre compatible with sufficient strength." The mode of using this instrument is the following:—Having attached the pervious needle to the exhausted pump, it is introduced sufficiently far from the surface to ensure the lateral apertures being completely covered. The cock next the needle is now opened, and the latter is pushed onwards until it reaches the interior of the cavity to be emptied, when the fluid rushes with rapidity into the vacuum prepared for its reception. This cock is now closed, and the outer one opened to allow of the fluid being evacuated from the chamber. Both cocks are now closed, and the process is repeated until sufficient fluid has been removed. In using the trocar and cannula, the pump cannot necessarily be attached until the former has been withdrawn.

The following case, in which I employed the aspirator, proved most satisfactory:—

On the night of the 5th of last November, I was requested by my colleague, Dr. Stokes, to perform thoracentesis upon a patient of his, who was labouring under intense effusion into the left pleural cavity. As the patient was in imminent danger, and the aspirator was not at hand, I introduced an ordinary sized trocar and cannula, and evacuated $6\frac{3}{4}$ pints of serous liquid. I did not remove the entire, as I feared the entrance of air, and as the patient expressed himself so much relieved. On the 11th of the same month the effusion had re-accumulated. I accordingly removed, by the trocar and cannula attached to the aspirator, 8 pints of fluid—this time completely emptying the chest. No further effusion took place, and three weeks afterwards, with the exception of a contracted side, no evidence of the late pleurisy existed.

I may mention, in connexion with the latter tapping, a curious physiological fact, viz., that the last pint of the fluid coagulated with much greater rapidity than the preceding portions, a satisfactory explanation of which phenomenon it is difficult to find. In paracentesic thoracis, I am decidedly of opinion that it is more judicious to employ the trocar and cannula in connexion with the

aspirator, than the tubular needle; as thus the danger of wounding the re-expanding lung, or other important structures, is entirely avoided; while in puncturing diseased joints, or chronic abscesses, the hollow needle is equally efficacious and quite unobjectionable. In a case of total retention of urine, my colleague, Mr. Porter, lately tapped the bladder above the pubis most successfully with this instrument, the operation not being followed by a single bad symptom; and I have seen my friend, Mr. White, equally happy in the treatment of a large psoas abscess associated with Pott's caries, by the same means.

Art. XI.—*Laryngismus Stridulus.* By Lawson Tait, F.R.C.S., L.R.C.P., &c., Birmingham.

The affection, or morbid conditions and their concomitant group of symptoms now known by the name of laryngismus stridulus, has more or less engaged the attention of all authors who have noticed such diseases; and although it is only in very recent times that any correct ideas of its pathology have been entertained, yet its semeiology and etiology have been tolerably well known, at best, however, only after a confused fashion, since the earliest times of medical literature. In the Hippocratic writings mention is made of an asthma as a disease of childhood; and as an evidence of how accurately the Father of medicine noted symptoms and how shrewdly he laid connexions (although in this case he was in error), he imagined the sudden attack of this asthma to be in some way connected with that great affection from the gods *την ἱερην νουσον*, and really the appearance of the paroxysms, their sudden appearance and disappearance would seem to justify his *à priori* views of its pathology.[a] Nearly all the subsequent writers on medicine notice such cases more or less minutely, giving more or less feasible explanations of their occurrence. In 1668 we find it pretty well described by a Spanish author, De Fonseca, in his treatise *De Angina et Garotillo Puerorum*, published at Madrid. Later in the same century we find Felix Plater describing a case most faithfully and minutely. It is also mentioned in Dr. Richard

[a] Trousseau (Clinique Medicale, Art. Epilepsie) says that he thinks these convulsions may be indicative of epilepsy, as, sooner or later, they are attended with more distinct and characteristic phenomena. As I have never seen epilepsy in infancy, I am inclined to believe that this great modern physician, who saw epilepsy where others have failed to find it, has pushed one of his favourite views a little too far.

Gower's *Treatise on Diseases of Children*, published at London in 1682, and by Dr. John Pechy in 1697. Our great English physician describes a disease which he, following Hippocrates, ascribes to epilepsy, and which is most probably laryngismus stridulus:—"Sometimes the fit comes suddenly, distorting the mouth and eyes, causing the face to turn black, and convulsing the limbs. Sometimes the fit is known to be coming by an involuntary contraction of the finger into the palm of the hand and an uncommon fixedness of the eyes." Hoffman well describes the disease in the sentence, "Tussis saepe firma et sicca infantes exercet, cum magna respirandi difficultate et suffocationis metû." In 1761, Dr. James Simpson published *De Asthmate Infantum Spasmodico*, with a most accurate description of the symptoms; mentioning the peculiar sound, "cum clangore surdo" (the best translation of *surdo* in this instance is *indescribable*, and so, truly, this peculiar sound is, but once heard it is never to be forgotten); that the disease continues its course without cough, and that it sometimes destroys the patient in a single attack. Dr. John Millar describes a disease and identifies it with the spasmodic asthma of Simpson, and remarks a tendency to glandular disease and general strumous condition. Rush, the great American physician, and the friend of Washington, wrote a paper confirming Simpson's views as to the condition being an acute asthma, but declaring that the pathological appearances were entirely negative, thus differing to a considerable extent from Millar; although, in fact, they are both to some extent right. Dr. Haygarth, of Chester, gives an instance of the amount of confusion existing in the minds of many writers on this disease, when he tells us that Dr. Richard Russell, in his book *The Economy of Nature in Acute and Chronical Diseases of the Glands*, Lond., 1755, was the first to mention the convulsive asthma of children. In addition, Dr. Haygarth does not seem to have learned to distinguish it from true croup.

Of the term laryngismus stridulus, I may say that it has little to recommend it, save its convenience and the general acceptance it has met with, for it makes no attempt whatever to classify the disease further than to indicate the larynx as a seat of a symptom; while no correct idea is conveyed as to what that symptom really is by the word *stridulus*. The sound emitted during an attack of the disease is not stridulant or creaking; it is much more accurately described by the English name for the affection, *the crowing*

inspiration, but I would much prefer to say that it was, to use Simpson's expression, a *clangor surdus*, an indescribable sound. Various names have been proposed for this affection, but all open to objection. Guersant, the writer of the article "Croup" in the French Dictionary of Medicine, has proposed the nomenclature of *laryngitis stridula*, as opposed to *laryngitis pseudo-membranacea*, or true croup, but I need not say that such a term is totally inapplicable, as the disease is not one of local inflammatory origin. The use of such a name has arisen from the mistake that many have fallen into of considering this affection as merely the first stage of true inflammatory, exudative croup—a mistake which, strange to say, Dr. West gives his adherence to. The term *thymic asthma* has been proposed by some because they had happened to find in one or two cases the thymus gland unusually large.[a] To this term I shall have further occasion to refer. *Cynanche stridula* and *suffocatio stridula* have also been proposed, the latter with more reason perhaps than any other. The disease has been also vaguely defined by Clarke, in his *Commentaries*, as "a peculiar species of convulsions in infant children;" and by John Cheyne as "spasmodic croup."

The last term is that adopted by William Henry Porter, the distinguished Irish surgeon, and in detailing to you the symptoms of this affection I cannot do better than read to you the description of the disease to be found in his admirable work on the *Larynx and Trachea*, written in 1826:—

"It is in general a diseased action consequent on irritation, and should be considered rather as a symptom than as a disease itself. It occurs in very young children, and appears at the commencement of the period of dentition; sometimes earlier, but very rarely after the child has reached its third year. In some instances the constriction is both slight and momentary, and the spasm never produces either unpleasant or dangerous consequences; in others it is longer continued, and the infant struggles for breath, becomes purple in the face, and is apparently dying when the spasm begins to relax, and a long-drawn, crowing inspiration sets all things to

[a] Dr. Hirsch, of Königsberg (Denkwurrdigkeiten in der ärtzlichen Praxis, Frankfurt, 1830), describes many cases where the thymus was enlarged, and he gives the disease the name of the asthma Kopp, believing that the latter author had first attributed the disease to that cause, although Hood had done so long before (Ed. Med. Journal).

Dr. Kyll (Rust Magazin fur die Gesammte Heilkunde, 1838), narrates cases of, and asserts his belief in, the thymic asthma.

rights again; and after a little time the patient revives and recovers well. These results are often mistaken for true (cerebral) convulsions, and form no inconsiderable proportion of the fits said to be occasioned by teething. In the fatal cases of spasmodic croup death is extremely sudden: the infant may be apparently in good health, and in a moment he makes a violent effort to inspire, which occasions something like a faint scream or cry, becomes black and swollen in the countenance, and dies before assistance can be procured." This is a description of that part of the train of symptoms to relieve which the surgeon's attention is directed; but of course there are others in which the physician is more particularly interested and which are part and parcel of the disease. Foremost and most constant in their attendance are those carpo-pedal contractions;—in the hand, a firm closure of the thumb upon the palm by the fingers, and extreme pronation of the entire limb; in the foot, bending of the toes, especially the great toe, downwards, and inversion of the foot. This is, as I have said, a very constant symptom. It has been present in all the cases I have seen, and I have as yet heard of no case where it was noticed to be absent. This symptom is, however, not peculiar to cases of laryngismus stridulus, but is a constant symptom of all head affections in children; to this point I shall have occasion again to refer when speaking of the pathology of this disease. After the fit is over the peculiar sound made and free respiration once more established, the child begins to cry, and generally falls asleep in a few minutes, exhausted. During this sleep the carpo-pedal contractions continue. There are other symptoms common to this and the other cerebral affections of infancy which frequently occur, such as vomiting, squinting, convulsions, &c. These are the positive symptoms. The negative indications are absence of fever and perfect freedom of respiration between the attacks, and that the progress of the disease is unattended by cough. Indeed, unless there be the more decisive evidences of brain disease little will be noticed, save that the child is fretful and evidently in imperfect health, though most of the victims present more or less distinct evidence of a strumous habit. When considering the etiology of the disease we find the first ruling condition is age. It is entirely a disease of infancy. Ley gives as reasons for this that the crusta lactea and excoriations behind the ears are prevalent during the time to which this disease seems limited; but these conditions are not confined to children under three years of age, while we find

that laryngismus stridulus practically is so. He barely hints at the true cause which is to be found in the relatively small size of the larynx. "When the inferior or recurrent laryngeals are cut, compressed, or irritated," writes John Reid, "all the movements of the muscles of the arytenoid cartilages are arrested, and the upper aperture of the larynx can no longer be dilated during respiration. If this happen in an adult animal, when the capacity of the larynx is comparatively large, respiration may be carried on with little difficulty; but if it should take place in a young animal, when the larynx is proportionally much smaller, difficulty of respiration is created, a struggle for breath takes place, and the very struggles increase the difficulty, as the increased force of current brings the arytenoid surfaces in apposition, thus mechanically decreasing the aperture while the inherent power for dilatation is gone." Thus we find that the prominent symptom of the affection is mechanically produced in a manner which we shall afterwards consider. This symptom may be imitated to a certain extent by any one during an inspiration, but not in expiration, and herein consists a most practical distinction between this disease and true inflammatory croup, that in the one the difficulty is during inspiration *alone*, while in the other expiration and inspiration is equally affected. Another predisposing cause for the occurrence of this is its hereditary tendency, or in other words, its strumous nature; as Dr. Ley has it:—"It may be doubted, however, whether this constitutional proneness does not resolve itself into a mere disposition to scrofula. My own experience would justify my deciding in the affirmative; and this opinion derives material support from the fact that the causes which thus aggravate or produce the disposition to the crowing inspiration of children are precisely those which call into action a strumous diathesis." Dr. Marsh has written that all the cases of this disease which he had witnessed had occurred in children either themselves exhibiting marks of the strumous diathesis or sprung from scrofulous parents. This is most certainly my own experience in the matter, and we find it supported by further majority of observers of the disease, especially in a most excellent essay by Dr. Wm. Griffin, in the *Dublin Medical Journal* for 1838, at the end of which he sums up his opinion by saying that authorities concur in asserting that for the most part, if not wholly, its occurrence is limited to those of strumous habits, and in families where there is a tendency to head affections and convulsions.

We find in addition that the exciting causes are moist climate and uncertain temperatures, irregular or bad food, and bad air; just the conditions, indeed, which influence, as exciting causes, the production of all strumous diseases, of which laryngismus stridulus is essentially one.

Regarding the pathology of this affection great confusion has existed, and indeed still exists, as may be seen from the various names applied to it, and from the fact that some authors, as Dr. West, deny it a separate nosological existence at all. To Dr. John Clarke is due the credit of first regarding it as a distinct disease, and claiming for it as such a place in our tables. Some of the old names for croup show that there has been great confusion between the exudature cynanche trachealis and laryngismus stridulus. Were *croup* used as a general name, as I think it ought to be, the term *spasmodic croup* would be the least objectionable of all the names which have been employed.

The word croup is derived from a Moeso-Gothic word *krop-jan*, degenerated in Icelandic to *krop-a*, signifying to make a noise with the throat. This, through Scandinavian handing down, rested in the eastern and northern parts of Scotland in the form of *croup* or *roup*, both of which were applied in the old Scotch language to the noise made by crows and frogs. In some old chronicle it is used in this way—"The ropeen of the rauyuis gart the cras crope," and in *Hudson's Judith* mention is made of the *crowping* of frogs. The adjective *roupy* is still in very common use among the Scotch peasantry, and is applied almost exclusively to the noise of croup or other form of impeded respiration, but I have heard it applied to the noise of owls. Knox, in his history, referring to some of his foes as limbs of Satan, says, they "*roupit* as they had been ravenis." Later we find that in such writers as Beattie and Fergusson the words are confined to the disease:—

"Oh! may the roupe ne'er roust thy weason (trachea),"

says Beattie; and Fergusson writes probably in allusion to some form of incantation for frightening away the disease—

"Baith cooks and scullions mony ane,
Wad gar the pots and kettles tingle,
To fleg (frighten) frae a' your craigs the roupe."

Thus there could be no objection to make croup the generic term, while the difference between the varieties would be sufficiently

defined by the words *inflammatory* and *spasmodic*. The term cerebral croup has been employed by several writers, and amongst others by Mr. Pretty, the only author, it seems to me, who has really conceived the true pathological condition of this disease, or at least of the great majority of the cases described under the name of spasmodic croup or laryngismus stridulus. Dr. Cox, in his work on *Convulsions Peculiar to Children*, Lond., 1825, records his opinion, after seeing many cases, that it ought to be named cerebral croup. Dr. D. Condie, of New York, in his *Practical Treatise on the Diseases of Children*, 1844, says that when it occurs in a child over two years of age, it is almost invariably symptomatic of cerebral or cerebro-spinal affection. Other writers have termed it chronic croup. It is very curious to observe what confusion and contradiction exists in the writings even of one and the same author regarding this disease. If we take Dr. John Cheyne, in the *Encyc. Prac. Med.*, as an example, we find that eminent physician, writing so recently as 1833, stating in one paragraph, that "the symptoms of croup which depend upon stricture of the glottis differ so in degree as to prove in many attacks the existence of spasm; this, however, is spasm caused by inflammation, for which no antispasmodic will be equal to venesection. That inflammation in this disease is often spontaneously resolved, the spasm depending upon it subsiding at the same time, is unquestionable; and has given rise to a belief, entertained by those chiefly who have overlooked the evidences of inflammation, that there is a purely spasmodic species of croup; we therefore take the opportunity which presents itself of stating that the hysterical affection just alluded to is the only species of spasmodic croup with which we are acquainted." The hysterical affection here referred to is the occasional laboured inspiration—half groan, half sigh, which we see young women indulge in during a fit of hysteria, and which has as much right to be classed with croup as has small-pox. It may excite surprise to read of Dr. Cheyne talking so gravely about the appearances of inflammation vanishing so instantaneously, but when another paragraph on the same subject is read, the surprise will, I am sure, not be decreased. After enumerating the symptoms of laryngismus and describing them accurately, he says:—"This affection, which ought never to be called croup, is not attended with cough; on dissection there is no trace of inflammation in the windpipe; the seat of disease, it can scarcely be doubted, is the brain"—and yet in spite of his

recognizing or rather guessing at the real pathological condition, naming the group of symptoms "a well-known disease of infancy," and taking notice of the characteristic sign, "the clubbing of the hands and feet," he quotes Dr. Kellies' well-known *Letter on the Pathology of the Larynx and Bronchial*, to the effect "that there is truly no essential difference between them (*i.e.*, the group of symptoms already described by himself, and now known as laryngismus stridulus, with true inflammatory croup), other than what arises from degrees of violence; and the obvious circumstance of intermission and continuance." But, alas! for Dr. Kellie's logic, his latter saving clause destroys all that he and Dr. Cheyne have already said on the matter. Even now the confusion has not yet ceased, but in the last volume of *Guy's Hospital Reports* has reached a new phase, under the name of "Tetany or Remittent Tetanus," in a paper by Dr. Moxon, where he narrates a well-marked case of laryngismus stridulus under that new fanciful name. Dr. Moxon tells us that the mother stated that all her children who survived at birth had "inward fits," and one, a boy, had outward fits and "spasmodic asthma." The little patient, three-and-a-half years old, had carpo-pedal contractions, difficulty of breathing, with croupy sounds, and at his first visit to Guy's, was seen by some one else, who, I think, properly named the case to be one of "spasmodic croup." Dr. Moxon does not agree with this propriety, but does not show good reason why.

There are three views of the pathology of this disease which I wish to bring under your notice: the first is that advanced by Dr. Ley, in his elaborate work on *Laryngismus Stridulus*, where he maintains that the spasmodic attacks are due to pressure on the vagus or its branch, the recurrent or inferior laryngeal by enlarged glands, creating an interference with the function of the nerves, and resulting in paralysis of the dilating muscles of the larynx, which he imagines are alone governed by that nerve. The second is that of Dr. Marshall Hall, who, in his struggles to account for every thing by what he called reflex motion or action, included, in his attempt, this disease, and stated that the attacks of spasm were caused by irritation causing reflex action. This, he said, originates in three nerves: 1st, the trifacial in teething; 2nd, the pneumogastric in overfed or improperly fed infants; and 3rd, the spinal nerves in constipation, vermes, catharsis, &c., these nerves acting through the spinal nerve and the recurrent laryngeal, and the motors of respiration. The third theory is one which is only

touched upon incidentally, or hinted at by most writers, and only thoroughly advanced by very few, including Mr. Pretty, although all, even Ley himself, advanced numerous facts in its support. It is that the disease is essentially one, in nearly every case, of cerebral origin, depending usually upon a tubercular inflammation of the meninges at the base of the brain. Dr. Ley's theory is based upon a very superficial and most essentially erroneous information regarding the physiology of respiration. The recurrent laryngeal supplies *all* the muscles of the larynx, both dilating and contracting, except the crico-thyroid; it is not, as he supposes, distributed only to the dilating muscles, so that paralysis of that nerve would leave the closing muscles in full power without any antagonism; such a state of matters would render expiration almost as inspiration, which we know in this disease is not the case. It was clearly shown by Le Gallois and confirmed by John Reid, that the distribution is as I have said, when the recurrent is divided the larynx is completely paralysed, and that the difficulty of inspiration in laryngismus is occasioned by the valvular wedge-shaped entrance to the larynx being readily closed by a forcible *down current* of air. Moreover, as the pressure which Dr. Ley presumes to be the cause of the mischief is necessarily permanent, the resultant paralysis or partial paralysis would necessarily be permanent also, and so, of course, would be the difficulty of breathing, and not as it really is, spasmodic with perfectly free breathing between the attacks. This is shown to be the case where there is no doubt about the pressure, as in cases of aneurism, or such cases as one narrated by Professor Henderson, where there was suppuration along the courses of both pneumogastric, and where the difficulty of breathing once established was constant, and the sound evincing that the functions of the nerves were exerted was heard during expiration as well as inspiration.

Besides all this, the facts of the morbid anatomy do not bear out Dr. Ley's theory: many, even some of his own cases, militate against it. It is not by any means the rule that we find enlarged glands in cases of laryngismus. I believe it to be the exception, because among the cases I have seen I have not been able to detect them, and in none of the then dissections I have made of children destroyed in an attack did I find a single enlarged gland of the slightest consequence. How many dozens of cases do we see where the glands in the neck and thorax are enlarged, and where there is no symptom of interference with the breathing? Last

winter I had charge of a poor child in whose body every gland that could contain tubercle was crammed with it, so much so that the right arm and the right side of his face were enormously œdematous on account of obstructed venous return, and yet he never had a laryngeal symptom.

When enlarged glands accompany laryngismus they are entirely a subsidiary phenomenon, depending on the same predisposing cause, struma; or they may be, as Cruickshank, in his observations on the Glandulæ Concatenatæ, says, they often are depending upon the disease of the brain. In many of the cases which Ley quotes, he confesses that others had differed with him in his opinion about the influence of the enlarged glands, and had expressed their convictions that the symptoms were due to meningeal affections. In one case the child had convulsions, and that down-cast of the eyes which compels the patient to raise his whole head to look at an object, and which is purely indicative of brain disease. At the examination he found the enlarged glands in the neck, and, of course, attributed everything to them. "There was, however," he says, "thickness and opacity of the arachnoid, with some degree of infiltration of the pia mater, especially in the situation of the optic nerves"—the usual situation of strumous meningitis; and yet this effusion he imagined to have been the result of the attacks of asphyxia and crowing—a strange example of the ὕστερον πρότερον.

It would be tedious to enter into an analysis of all Dr. Ley's cases, but in the majority of them there are points which render them valueless in support of his views. In one, he says, that the recurrent on the right side was found embedded in the midst of the enlarged bronchial glands, an anatomical relationship, which would be so peculiar, that the correctness of the narration may be doubted.

Then, again, the almost constant presence of the carpo-pedal contractions detracts very much from the probability of Dr. Ley's theory, as it is not clear what influence glands in the neck or chest can have upon the extremities; while the presence of these symptoms, in nearly all undoubted cases of head affection, is a strong *à priori* argument for considering laryngismus stridulus one of that group.

Equally fallacious seems the theory that this disease is caused by pressure on the nerves by the thymus gland; but so prevalent has been this idea that, as I have already said, the name of thymic asthma was proposed for the affection, and under that name you

will find a case recorded in Dr. West's book on *Diseases of Children*, in which the weight of the gland is stated to have been 328½ grains, and the pressure of the gland on the pneumogastric nerves was supposed to be the cause of the symptoms and their result. If we bear in mind the relations of the thymus gland, that it lies in front of the trachea, and that its lateral boundaries, according to Sir Astley Cooper, are the reflections of the pleura, and that it reaches from about the 4th bone of the sternum up to about half an inch below the lower margin of the thyroid body, it is difficult to see what it can have to do with the vagus or the recurrent. When we consider likewise that at birth the gland weighs normally about 250 grains, and that it increases in size up till the age at which Dr. West's patient died, and the fact mentioned by him that the brain in the case referred to presented appearances of disease, I think we are bound entirely to preclude the idea that the gland was either larger than it should have been, or that it had anything to do with the disease whatever. The conclusion at which I have arrived is that it was an ordinary case of strumous spasmodic croup. Did the thymus gland ever evince any tendency to mischievous pressure, the first evidence of it would certainly be that of obstructed venous return.

Dr. Marshall Hall's theory has little to recommend it, save that an occasional case may occur where there is no other explanation to be offered than that of reflex irritation. We all know from experience that there frequently is a connexion established of some sort or other, between the pain and irritation of the mouth, in teething, and gastro-intestinal disorder, and often between irritation or disorder of the bowels, and the apthous mouth, and even with some amount of cerebral disturbance; but, on the other hand, how many hundreds of cases do we see of very severe suffering in teething, and from intestinal disorder, without meeting with a case of laryngismus ; and again, how frequently we meet with cases of laryngismus without any irritation of any kind being observable.*

* I am enabled here to add a very interesting note from Sir William Jenner's Lectures on Rickets, for which I am indebted to the courtesy of my friend Dr. Russell, Physician to the General Hospital :—

"The connexion between rickets and laryngismus stridulus is very close. I think it is about four years ago that I was struck by the connexion between them, and since that time I have seen a vast number of cases of laryngismus, and in every case, saving two, the child was the subject of rickets ; and I believe the reason of laryngismus stridulus being so constantly referred to the irritation of teething, is that the ricketty

Dr. Cheyne, in his well-known essays on *Hydrocephalus*, mentions the crowing respiration, *not followed by cough*, with the clubbing spasm of the hands and feet, adverting to this peculiar affection as connected with the diagnosis of the disease of which he treats. Dr. Ley, in mentioning this passage of Dr. Cheyne's writings, says that that distinguished physician does not connect the two affections; but, to my mind, it is very clear that he does connect them very intimately in the relation of symptom and lesion.

Leopold Anthony Golis, in his work on *Acute Hydrocephalus*, suggests that laryngismus (acute asthma is the term he employs to designate it), is one of the predisposing causes of acute hydrocephalus. In this he shows an accurate observation of the connexion between them; his mistake is that what he imagines to be the cause, is merely a symptom.

John Burns, the celebrated obstetrician of Glasgow, gives the opinion "that cerebral disease is more intimately connected with the spasmodic form of croup than many imagine;" and seems to think that though this disease may sometimes have a reflex origin, yet, that it generally arises from a state of temporary paralysis of the recurrent from a cerebral affection at the base. "This sudden and, perhaps, transient attack of spasmodic croup, requires constant attention, as it is often the prelude of incurable disease in the head or spinal sheath, and is more immediately alarming if complicated with, or succeeded by general convulsions. It is too often connected with an inflammatory, or highly disordered state of the origin of the nerves coming off at the base of the brain, and this points out the demand for imperative treatment."

Pretty describes, as cases of cerebral croup, many cases of this disease, three of them occurring in his own family, and however much it is to be regretted that he was so unfortunate as to suffer to such an extent in his domestic circle, yet it has had the advantage of having given us a more faithful, trustworthy, and minute account of cases of the disease, than could have been procured by any other circumstances. The first of his children suffered for many weeks, at uncertain intervals, from laryngeal spasms, accompanied by and ending in croupy inspirations, but was apparently otherwise in the

condition retards the development of the teeth, and the practitioner refers the laryngismus to that which, like itself, is the consequence of the constitutional disease. Carpo-pedal contractions and even general convulsions are not, as is well-known, unfrequent in these cases; and are, like the laryngismus, to be referred primarily to the irritability of the nervous system and muscular debility."—Medical Times and Gazette, May, 1860.

enjoyment of perfect health. One of these attacks killed her instantaneously. Upon dissection the blood-vessels of the brain were found unusually loaded, and effusion had taken place into the ventricles, and between the arachnoid and pia mater. Four intussusceptions of the ilium, without any evidence of local inflammation, were also discovered, but no other appearance of disease. (These intussusceptions were merely instances of what we so frequently find in cases of sudden death, when one, two, or more invaginations of the small intestines occur either during the agony or immediately after death. This, I believe, to be peculiarly common in children). His second child was seized with convulsions a week after the death of her sister (they were twins, an important point in tracing the predisposing cause, struma), and had repeated paroxysms; the more advanced symptoms of meningitis came on, such as the peculiar motion of the arm about the head, frequent startings, with screaming, insensibility, squinting, and dilated pupil, &c. This child recovered after having had leeches applied to the temples, blisters to the head and neck, and the administration of the mistura assafœtida. The age of these twins was eight months. A third child of Mr. Pretty, a boy, was taken ill, when seven months old, in a precisely similar manner, but his case ended in recovery. As Mr. Pretty details them, no series of cases could be more instructive, as bearing on all the points connected with this disease, than those so carefully, and no doubt painfully, narrated by the father of the patients; bearing, as their histories do, upon its causes and symptoms, its pathology and treatment. "I asked," says Mr. Pretty, "a great many medical friends about the disease, but got little satisfaction. One said it was from cutting a tooth; he lanced the gums, but no tooth came till two months after recovery had taken place."

Besides those of his own family, he narrates about a dozen cases which had been under his own care, and, in all, he had strong reasons for believing that the encephalon was the seat of the disease.

The following facts, all of which have occurred in very strongly marked instances of laryngismus, under my own notice, also tend very much to strengthen the view of its pathology, which I have endeavoured to support in this paper. These spasmodic attacks are almost invariably the precursors of general convulsions and the other decided indications of cerebral disease, if they continue long enough, either from the fits of suffocation not proving fatal, or the means

adopted for their removal not proving effectual. It is very common for children to die of this affection in families in which other children have died of marked strumous brain disease, either complicated or not with the crowing inspiration, or where there is in the family a marked tendency to tubercular or strumous diseases. Pretty mentions the case of a man, father of two children, who died of this affection, who had always shown a weakness of intellect, and who ultimately became insane; when we recollect the intimate connexion between the strumous and tubercular diatheses and insanity the case becomes one of importance.

The three cases in which I had opportunities of examining the condition of matters after death, bear out what I believe to be the true view of the pathology of this disease. The first was the case of a male child, about seventeen months old, which I was asked to see by Dr. Gordon, of George's-square, Edinburgh. He had suffered from five severe attacks of spasm before I saw him, in each of which his mother said she expected to see him die in her arms. On my second visit I happened to see him in one, and certainly I have seldom seen anything nearer total suffocation than that poor child recovered from. I proposed that tracheotomy should be performed to prevent the attacks proving fatal. This was agreed to by Dr. Gordon, but declined by the parents. The child died two days after this, and permission was granted to examine the body. The dissection I performed in the most careful manner, laid bare *in situ* the entire length of both pneumogastrics in the neck and thorax, and ascertained that there was not a single enlarged gland to be found any where; neither were the pneumogastrics and their branches, the recurrent laryngeals, interfered with by any pressure, nor did they present any altered appearance. All the organs, both abdominal and thoracic, were healthy. The bronchial glands were so small as to be found with difficulty. In the brain was found ample evidence of strumous meningitis, and the ventricles contained above the average amount of fluid. This child had a strumous family history; in fact, there was no need to go further into this than to ask to see the father and mother. Convinced that had tracheotomy been performed, this child would have had the chances of his life very much increased in his favour, I have proposed it in two other fatal cases, with which I have met, but in both it was refused. In one of them, a female child, thirteen months old, similar negative appearances were met with as in the first, while the evidence of cerebral disease was much more distinct, as she had been labouring

under a chronic form of it for several months. In the third case, a female child, nine months old, the cerebral disease was well marked; while the only other morbid appearance was that of broncho-pneumonia, the symptoms of which had come on after the laryngismus had made its appearance, and were induced by the poor child having been carried by its drunken mother through a severe snow storm. This young sufferer had no very distinct history of struma; but the facts that its mother was a drunken collier woman, and its paternity doubtful, were quite sufficient to account for a strumous habit or, at least, a strumous disease.

I have used the word struma instead of tubercle throughout this paper when speaking of the meningeal lesion which is known as tubercular meningitis, strumous meningitis, or simply acute meningitis. The *post-mortem* appearances would entitle it to the first of these terms, but since it is a matter of every-day observation that this disease occurs in children in whose family history we have indications of the strumous diathesis, which in those spring from a purely tubercular stock, its frequency is far from being well marked, I prefer the second of the names. Between the strumous and tuberculous diathesis I do not know that I am in a position to give a satisfactory pathological distinction, but for clinical purposes they are sufficiently distinct.

There are other views of the pathology of laryngismus stridulus, most of them, however, too eccentric to demand serious attention. Such are Dr. Hood's view, that it depended on fatty degeneration of the liver, and that of Elsässis, that it was due to thinness of the calvarium. It was, I believe, Prof. Rolleston who showed that fatty liver in childhood is essentially a strumous disease, and if there be a connexion between this lesion and laryngismus stridulus I must claim for them community of sequence.

The treatment of cases of laryngismus stridulus has undergone as much variety as might be expected in a disease so mysterious, and the pathology of which has been so misunderstood. It must be perfectly evident from what I have said regarding its etiology that the heroic anti-phlogistics, bleeding and low diet, are inadmissible. Childrens' diseases, as a rule, do not stand bleeding, and they very rarely require it; and in the instance of laryngismus the patients certainly will not benefit by it. Good nourishing, though at the same time easily digested, food, with even slight stimulants, should be had recourse to. There is nothing that I have found to equal whipped white of egg with a little milk and

sugar, uncooked; and, if a stimulant should be required, the addition of a dessert-spoonful of champagne or a tea-spoonful of brandy. There is no stimulant that children more readily take than champagne, and none which seems to be of more service to them. I have very often, I believe, saved children by this wine when they have refused all other stimulants. Before administering it the effervescence should clear and an equal bulk of water be added; if the patient be feverish and it seem grateful, no fear need be entertained about the addition of a little ice. Milk and animal soups will also be of service. Counter-irritation on the head I have often seen of great use; and the forms which seem to me most convenient is the inunction with equal parts of croton oil and glycerine of the occiput, or an iodine blister. Of internal remedies that from the use of which I have seen most good accrue is, without doubt, iodine; or iodine in conjunction with bromine. In the form of iodide of potassium alone or combined with the bromide I am certain that I have seen it check most serious cases. The bromide of potassium especially seems to have a powerful sedative action on the nervous system, and in this way often checks or alleviates the spasms. To effect this, however, it requires to be given in large doses, and to a child of a year old I have given as much as two and a half grains of bromide of potassium and half a grain of the iodide with marked effect. Grain and two grain doses of the iodide at the same age are not excessive. Of course such causes of suffering as apthous mouth, irritable gums, and diarrhœa must be attended to.

The inhalation of chloroform, if the time of warning will admit of it, will be found of great benefit, as there is no more valuable aid in the treatment of infantile convulsions.

The question of the performance of tracheotomy is serious, but one which I think ought to be entertained; it is not likely to be one to which parents will consent until too late, nor is it likely often to be the fate of a competent surgeon to be on the spot when the operation is urgently needed. The risks of the operation in its after-consequences are great, and amongst English surgeons I fear it is not likely to find favour.

ART. XII.—*Reports of Surgical Cases.* By MR. MORGAN, F.R.C.S.I.; Surgeon to Mercer's and to the Westmoreland Lock Hospitals, &c., Dublin.

Removal of a large Fibrous Tumour of the Face, which had caused protrusion of the Eyeball and the Upper Jaw—Successful result, and absence of deformity.

So much attention has been paid by operating surgeons to the successful removal of various face tumours, that the record of such cases is pretty abundant, specially where of an osseous nature, or with intimate osseous attachments; the pages of this Journal have, from time to time, abundantly illustrated these affections, and the happy results obtained by well applied surgical interference. I have much satisfaction in being able to add another, but diverse instance, where the patient has been relieved from great suffering and deformity, ending in a painful death, and is now in the enjoyment of excellent health, having worked all the summer and harvest, and being now in perfect condition. The deformity in this case was extreme, and the growth was steadily and progressively extending in various directions at the time of his coming under my care.

The patient, a strong-looking young man of nineteen, from the country, gave the account of his disease and the progress very definitely and clearly as follows:—In June, 1868, a tumour was first observed forming at the outer side of the left superior bone, and also slightly protruding the malar bone. This swelling was painless and felt hard. It remained very nearly stationary till November, 1868, when he found that it had increased and made its way under and above the zygoma, also shortly after he found it had made some progress towards the roof of the mouth, into which it protruded at the left side; this steady but continued increase progressed until May, 1869, when the molar teeth of the affected side became loosened and were extracted; he now sought surgical aid, in March, 1869, and the tumour at roof of the mouth was punctured, giving exit to a little thin bloody fluid. From this date the growth steadily and equably progressed, making its way so encroachingly, that he now was sent to Dublin. On examination, January 6th, 1870, the general appearance was most remarkable, and, as shown in the illustration, the distortion was very considerable.

The eye was now prominent and tense, and the sight was becoming

impaired. The tumour filled up over the zygoma, and protruded into the mouth to a very perceptible extent; the malar bone was also visibly displaced. There was no pain or tenderness on handling. The patient was very anxious about the increasing size, and the encroachment on his sight. He had no obstruction to the nostril, or difficulty of breathing.

On careful examination with the fingers passed in every available direction, the tumour seemed not to grow from the front part of the maxillary or malar bones, but gave rather the sensation that it grew from the inside of the anterior, or from its posterior wall, pressure on the tumour inside the mouth communicated with that part in the temporal region.

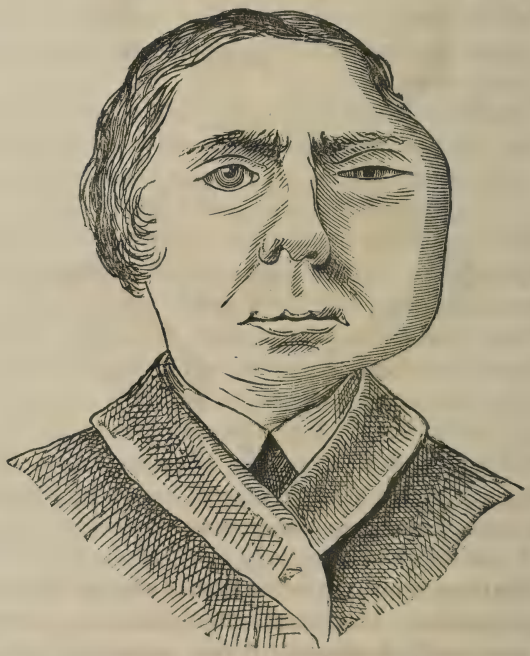
After careful consideration, it was determined to proceed with the operation of removing the tumour by exposing the maxillary bone, and leaving it an open question as to what might be most judicious to be done once the root or origin of the tumour was ascertained.

Accordingly, having explained to the patient what was required to be done, and that the use of chloroform was inadmissible, and having exhorted him to assist as far as possible by fortitude and endurance under the extremely trying circumstances of his case, I proceeded to operate.

Having fixed the patient in the operating chair, and retained him from moving, by passing a stout wooden bar across his thighs and under the arms of the chair, and having bound his hands so as to prevent his struggling or interfering with the operation. I carried a stout knife through the angle of the lip and prolonged the incision upwards towards the zygoma, the flaps were separated by assistants, and the tumour was explored; it was now felt closely lying against the maxillary bone, and coming from under the zygomatic arch, and it felt dense and resisting; by inserting my fingers and vigorously clearing away the mass, I found it impossible to reach it without dividing the malar bone. I accordingly carried up the incision a little further and nipped across the zygoma at its union with the malar bone, and again at its temporal extremity; this gave very great assistance, indeed, in the pursuit of the attachment of the growth. I now followed it inwards and backwards with my fingers, and found that it extended from the back of the maxillary tuberosity, and sprang apparently close to the ptergo-maxillary fossa by a comparatively narrowed attachment. I now seized the mass in a strong forceps and endeavoured to eradicate it; this I found impossible

to do, owing to its firm and deep attachment; fearing to use further force, I exposed the deep portion, passed a very stout ligature around it, and separated the tumour with the knife; the structure, during this section, felt firm and "criant:" there were several small vessels oozing from its surface, I therefore touched it over with a heated iron, which speedily restrained them; there was no particle of morbid growth left but the pedicle itself. The tumour was altogether condensed and in one, but had moulded itself around the various bones. The incisions were neatly drawn together by wire sutures, a plug having been introduced, with ligatures attached, to aid in its removal. The patient bore the operation with admirable fortitude, when its painful and difficult nature is considered. The hæmorrhage, in proportion to the size of the mass and the parts operated upon, was inconsiderable. The facial artery, which was cut across, was restrained by a ligature, and no other artery of any size was encountered. The internal maxillary could be felt pulsating at the deep attachment of the tumour.

Fig. 1.

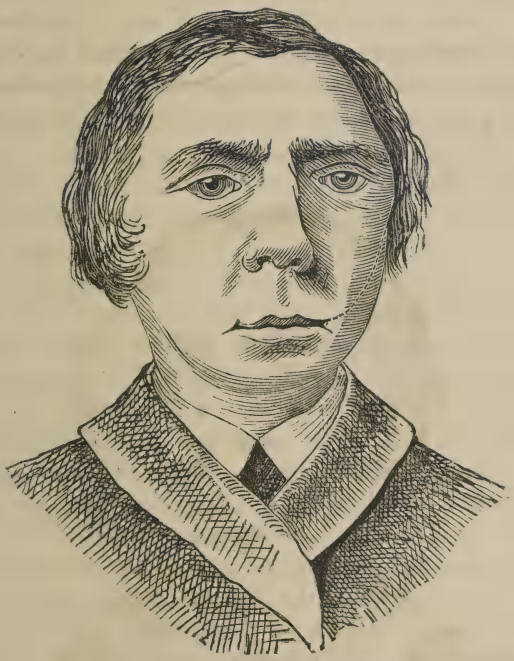


Remarkable deformity caused by the tumour of the face, as seen previous to operation.

The plug was removed in forty-eight hours subsequently. The patient had not a bad symptom of any kind. No salivary fistula, or inconvenience from an imperfect mouth. There was some paralysis and distortion of the mouth to the unoperated-on side, owing to the necessary section of the frontal dura nervous filaments; this, however, soon corrected itself, and no deformity beyond the cicatrical mark remains. The eye, which caused him so much, and very natural anxiety, recovered itself directly, and vision was perfect.

He returned to the country on March 19th, 1870, wonderfully restored.

Fig. 2.



The appearance after operation, the malar bone still slightly prominent, owing to the long-existing pressure.

The illustrations show the deformity caused by the growth, and the successful result. The malar bone remains prominent where it had been previously displaced by the progressive action of the tumour. The tumour, when examined, was firm and weighty, and

presented a curious outline, moulded, in fact, to the various positions it had occupied, having a kind of hour-glass off-shoot where it had been constricted by the zygomatic arch; and also showing where it had projected itself into the mouth. On section, it closely resembled the fibrous tumour found affecting the uterus. When a clean section was made through it, the structure showed fibrous dissepiments running here and there through the mass; and on microscopic examination, none of the oat-grained cells, referable to the fibrous recurrent tumour were found. The patient's aspect, so far from betokening anything of a malignant nature, was one of good health, making allowance for the pain and anxiety he had gone through. I have heard from him, last month, that he is in excellent health and condition.

Remarkable Malignant Tumours of the Head in a Child four years old, combined with extreme Exophthalmos.

The following case is of interest as illustrating the rapid and shocking effects of pressure by malignant growths. The unhappy subject of this disease, came under my care March 21st, 1870, and presented a most remarkable appearance, the eyes were both protruded to a frightful extent; in one of them, the cornea had completely sloughed, and the contents had escaped; in the other this condition was imminent, but had not established itself at the time of coming under my observation. The forehead felt puffy and thickened, and the tumours projecting from the surface were most peculiar, they felt elastic, and almost fluctuating; the skin was tightly drawn and shining; there was no discolouration of any kind; the veins, however, were enlarged, and marked out on the tumour in their course; they were as large as the veins on the forearm usually appear in a muscular man; the child, at the time of its admission, was sensible, and could indicate its wants and wishes very well. There was not the slightest deficiency of intelligence, or approximation to stupor, or insensibility. It cried out occasionally, and as far as could be ascertained, owing to pains shooting through the tumours; the stomach was not irritable, but the body was a good deal wasted and palid. The history given by its mother, an intelligent young woman, was thus:—

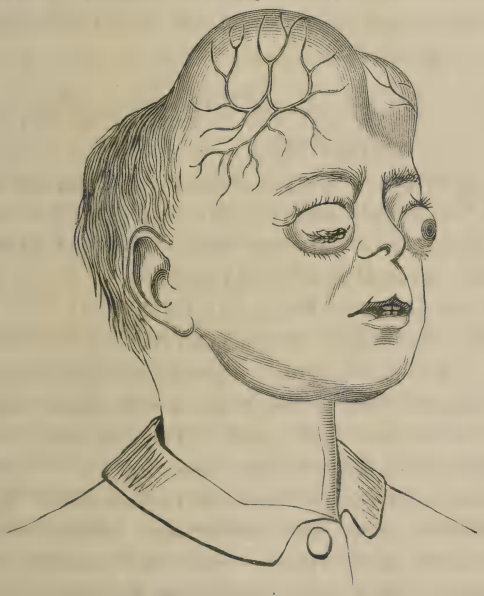
The girl was now aged four years and two months. It had been always remarkably healthy and intelligent. About five months previous to her admission under my care, she complained of pains referred to the thighs and legs; very soon, however, the right eye

commenced to be a little prominent, and a discolouration appeared over the eye and about the eyelid; the left eye, in about a month afterwards commenced to protrude also; the right tumour on the temple, was also about three months' standing and grew very rapidly, attaining its size in about three weeks; the tumour on the left temple commenced about three weeks after that on the right, and also grew rapidly, but not so rapidly as the right.

The child never had sick stomach; could walk perfectly well; was intelligent, and rather more so than children of her age.

At the time of death the child exhibited a pitiable condition from emaciation and the discharge from the eyes. She died on March 29th, 1870, from sheer exhaustion by suffering. As the case had been suspected in the early stage to be a variety of hydrocephalus, I was specially desirous of making a *post-mortem* examination, which I did by carefully raising the scalp, and removing the bone; underneath the integument of the forehead I found a considerable stratum of morbid growth, red, and vascular; this then extended upwards till it developed itself into the two remarkable tumours; it also made its way into the orbits, adhering to the bones, and thus by its development, protruding the eyeballs to the hideous and destructive extent shown in the illustration.

Fig. 3.



Remarkable malignant tumours of the head occurring in a child.

The right eye was completely collapsed, and the growth filled up to behind it tightly; the left was not so far destroyed; there seemed to be no intra-ocular growths.

On opening the skull the surface of the brain was perfectly normal; there was slight corresponding thickening of the dura mater bone; opposite to these points seemed thinner than usual, and the discolouration was visible on the inner surface. The *post mortem* examination proved that the disease was altogether between the cranium and integument, and seemed to have extended from the orbit upwards, till it formed these curious protrusions.

The growth partook of the nature of a fungus hæmatodes, being dark and vascular-looking in colour and consistence, with a certain remarkable feel during life of elasticity, which might by its similitude to fluctuating, have favoured the idea of the existence hydrocephalus; the intelligence of the child, the absence of nausea, and the perfect condition of the locomotory powers, together with the general history, led me, however, to pronounce the case during life as one of malignant growth, a diagnosis speedily verified.

The illustration gives a truthful idea of the affection. A cast of the head, and also the parts themselves, have been deposited in the Museum of the Royal College of Surgeons, Ireland. Plate V. in Mr. Pemberton's recent work of various forms of cancer, represents a somewhat similar case; and also Plate X. in Knapp's work, and described by him as glioma.

Amputation of the Leg for Idiopathic Gangrene—Successful result—Report of the Anatomical conditions of the diseased Extremities.

The following case is of surgical interest, showing that under some circumstances, the usually adopted rule of non-interference in cases of idiopathic gangrene may be relaxed, and a successful result obtained under inauspicious conditions.

There were, in this instance, certain hopeful indications; the patient, though about sixty-seven years of age, was a hale, strong man, in fact a "young old man;" he had enjoyed a life of continued good health; his circulation was healthy and vigorous for his time of life; the temperature of the limb was never lower than that of the corresponding one; the arteries of the opposite side, on examination by the fingers, felt perfectly healthy, as did also the femorals; but on the affected limb the anterior and posterior tibials at the ankle could be found apparently thickened, and these only not distinctly pulsating. The condition of the upper part of the limb was

healthy, and there was but slight œdema, or puffing of the cellular tissue. The pain complained of was always referable to one spot, to which the patient himself attributed all his ills, and the gangrene was at the time localized and quiescent.

H. M., a good specimen of the small farmer class of Ireland, aged about sixty-eight, came under my care the 9th of June, 1869; he had been always of most industrious habits engaged about his farm, and had enjoyed excellent health during his entire lifetime.

About December, 1868, he observed a hardish spot on the great toe point, something like a warty growth; this gave him no great pain, but began to increase in size, and in January, 1869, he got it lanced, giving exit to a little blood only. The entire foot soon became painful, and the toe a little discoloured at the top, and he continued in this position till March 17th, 1869. Meanwhile the extremity of the great toe had become more intensely discoloured, and he suffered much pain, especially at night. On the 23rd of March, he went to the County Infirmary, had the diseased extremity of the toe removed; this healed, but the second joint became diseased, and at his request it was removed also, now one month ago.

He now suffered most intense, constant, and wearing pain, both night and day; he has not lain down regularly for three months, but was constantly rocking the limb, and keeping it in the hanging position, as to allow it rest even on the bed, or on a level with the body, would be intolerably painful, though he was still robust for his age; the constant pain was beginning to tell on his strength and his patience; there was a slight blush redness over the front of the foot; the discoloured parts were cold and exhaling the peculiar recognizable gangrenous odour that is characteristic; there was no difference of temperature between the affected and the non-affected limb. The anterior and posterior tibial vessels could be felt pulsating, but manifestly with thickened coats, and, as it were, muffled pulsations.

The heart's action was good, and on careful examination, no disease in any part of the circulatory system was detectable; all the indications pointed to a localized obstruction to the perfect circulation in the limb, and it was curious to find that the vessels in the opposite leg had not the thickened feel that they had in the diseased one. The patient was most anxious for operative interference, and in fact came to town for the express purpose of having amputation

performed. He could no longer endure the excruciating and increasing pain of the part, and the loss of rest for night after night.

Having explained to him the dangers of the operation at his age, and those incidental to the diseased conditions which led to the gangrene, he still decided on the operation, as the pain was so trying and wearing. This was chiefly referred to the centre of the foot, and partially to the sole, and there was a special point indicated as the source of this pain.

The Operation.—I removed the leg as high up, below the knee, as was possible, leaving very long both anterior and posterior flaps, so that there would be the minimum of traction on the parts; the vessels, four in number, were secured by ligature passed well down and encircling some of the neighbouring tissues in the case of the tibial vessels which were thickened, and equalling about a No. 4 catheter; the flaps were easily and well drawn together, and without any tension, by means of wire sutures, supported by straps of adhesive plaster.

The patient, notwithstanding his age, bore the operation very well; there was but little oozing or loss of blood, and he was fully subjected to the influence of opium and quinine; the stump was kept warm, enveloped in wool, and in suitable position, to avoid pressure or tension of any kind. On the fourth day some of the cellular tissue on the front aspect, became discoloured and sloughed, and on the sixth day about half an inch of the margin of the posterior flap also sloughed; the parts were kept warm, dressed with camphorated spirit, and no further mischief ensued beyond these inconveniences, and the necessary delay caused in the cicatrizing of the flaps; the case went on most successfully, his condition improved, and he was able to return to the country, August 7, 1869, quite healed, and with a perfect, well covered stump. He has since then enjoyed excellent health for his time of life.

Examination of the Limb.—I made a careful examination of the amputated limb, and the main arteries were skilfully injected by Dr. Barker, at the Museum R. C. S. I. The injection would not run for the lower third of the leg, the vessels being impervious from about three inches above the ankles; immediately above this, the anterior and posterior tibial artery more particularly, would admit a surgeon's probe; the coats of the arteries were much thickened, but there was no ossification or specks to be found either with the magnifying glass, or by the touch; on splitting up the arteries as far as could be, the inner coat was found

adherent in many places, and in some parts bands of adhesions were bound across, glueing together and narrowing irregularly the walls of the arterial canal. In order to explain, if possible, the intense pain referred to the centre of the foot, which caused such misery, I made a careful section down the foot and leg, and found a most remarkable condition of the tarsal bones; on carrying the knife between the first and second metatarsal bones, the opposed surfaces of the inner and middle cruciform bones were seen to be completely disorganized; they were soft, intensely fœtid, and discoloured, the entire middle cuneiform, and the greater part of the inner was thus diseased, and also the corresponding extremity of the metatarsal bones of the great toe. The upper discoloured and gangrenous blotch, though somewhat corresponding in position to these parts, had no connexion with them; the other tissues were all healthy, and the gangrenous parts were disorganized down to the corresponding bones.

Fig. 4.

A section through the bones of the foot, showing the diseased condition of the bones.

The illustration shows the remarkable extent of the disorganization of the bones which took place.

The examination of the diseased conditions in this case is of much interest. The state of the arterial vessels bear out the theory propounded and so strongly advocated by Dupuytren, that there had been an inflammatory condition of the inner coating of the artery; in fact, an arteritis of a sub-acute adhesive character, showed its influence by occluding the anterior and posterior tibial arteries for fully one-third of their length; the fibular artery, though apparently enlarged

for the last two or three inches of its course, in like manner speedily became diminished in size, and closed at its lower part. Indeed this condition is adverted to by Cruveilhier (*Anatomie Pathologique*, Liv. xxvi.), where the arteries are described in two instances as blocked up by lymph; one of these instances is particularly remarkable where the anterior tibial was pervious, though the posterior and the fibular arteries were occluded; if the cases of spontaneous gangrene which have been published, be examined, it is curious that they seldom coincide as to the amount of obliteration; thus in one case the aorta common iliac, external and internal iliacs, popliteal, tibial, and fibular arteries were blocked up, yet a very limited gangrene confined to the toes of one foot only took place; in other instances, though the obstruction did not seem mechanically as great, yet a greater amount of mortification ensued.

In the examination of the arteries in the subject of these remarks there was no evidence of the calcareous degeneration alluded to by writers, and especially noticed by Cowper and Carswell; in tracing the vessels down through the lower part of the leg and foot, there was a gradual occlusion of a plastic, lymphy nature in the larger vessels, and the smaller seemed degenerated into mere fibrous cords; there was no osseous or earthy matter detectable, but the lymphy adhesions were in places apparently so recent, that they were capable of separation and unfolding, in the process of splitting up and examining the interior; the coats of the vessels were undoubtedly considerably thickened, and on section, when examined with a magnifying power, presented the appearance of a fibrous deposit and hypertrophy; this thickening was evident to the touch during the life of the limb, but did not by any means exceed the enlargement and dense sensation given in many elderly cases where no disease occurs.

Fig. 5.

Portion of the posterior tibial artery opened, showing the thickened coats and the plastic adhesions in the interior.

The cause of the pain is a matter also of some interest. It has been attributed to arteritis, as preliminary to and causative of the grangrene. In this case, I have reported, the pain was never, at any time, referred to the arterial canals, nor did it in any way indicate their course. The same region of the foot was constantly

pointed out by the patient, as the source of the wearing intolerable pain; and the subsequent examination, I have detailed, revealed a state of the parts, which explained easily the deep-seated mischief which told so heavily on the patient—keeping him for many weeks sleepless and in wretchedness.

As to the question of operation and its advantages in cases of idiopathic gangrene, dependent on internal causes beyond the reach of our art, or of our surgical interference, no doubt the general rule adopted has been, to avoid amputation in cases of idiopathic gangrene, adverted to by Pott, in his original description of these affections; and the great majority of surgeons eschew operating in general under such circumstances. There is, however, a certain difference of opinion as to how far this rule should be invariable. In our own city cases have occurred where amputation has been successfully performed. The *Dublin Journal of Medical Science*, Vol. iv., p. 19, contains cases given by the late Mr. Porter, where a limb was removed, with the best results, for gangrene, occurring three months after ligature of the femoral artery; and, again, from the pressure of a fungoid tumour of the periosteum. He also mentions successful operation in a case of Pott's gangrene of the foot. The *Archives Generales*, January, 1835, contain a case of similar gangrene operated on while it was spreading, yet the result was successful. Mr. Gross, in his masterly work on surgery, mentions that with regard to the question of amputation in such cases, it is extremely difficult to give a satisfactory statement, and that notwithstanding the usually observed rule of non-interference, "he had seen several cases where amputation was succeeded by the most happy results."

The general state of health of the patient must make a very great difference, indeed, in forming a decided prognosis; if the strength be much broken down by the gangrenous action, and by continued suffering, the chance is dubious; but as occasionally, just as in the case I have detailed, the patient is of unusual vigor and heartiness for his time of life, some prospect is given that under collateral favourable circumstances the operation will succeed. Nothing could be more satisfactory than the result in this case which I have detailed, the relief from the excruciating pain he suffered being in itself a sufficient recompense for the loss of the limb.

Aneurism of the Arteria Innominata—Apparent Cure.

A remarkable instance of consolidation of an aneurism, in so large a vessel as the arteria innominata, has been under my observation from July 8th, 1869. The man is now working every day, and apparently perfectly cured by consolidation. Dr. Gelston, Limerick, who examined him very carefully in December, 1870, one year and five months since I first saw him, states that now the tumour is absolutely consolidated, firm, and perfectly free from pulsation, and that the man is as well able to follow his occupation as he ever was.

P. F. was admitted under my care on July 8, 1869, an able-bodied man, aged forty, by occupation engaged in the salmon fishery, his chief occupation being the seine net-fishing, and he usually had the post assigned to him of holding the rope end of the net on shore; he was of necessity subject to a severe strain on the arms, and to this he attributed the origin of his disease; he has been always healthy; married eighteen years; and a sober, industrious man, father of ten children. About six months previously he got a severe pain in the point of the shoulder, which came rather suddenly, and shortly afterwards the root of the neck, at the right side, became affected, and he felt the "sinews" sore; in a few days after, as he was buttoning his shirt, he found a lump at the inside end of his collar bone; he showed it to some medical men, and finally, Dr. Thomas Gelston, of Limerick, saw it, who at once observed its nature, explained to the man his condition, and took him under his care, and after some time, the case being so important and interesting, he directed him, as he was coming to Dublin, to see me.

On examination, I found that the pain in the shoulder had increased to such an extent as to be now very unbearable, and that, in addition, he suffered from considerable difficulty of breathing, and also from inability to use the arm, which was so powerless, that he had been idle for some months, and quite unable to do any work.

He passed restless nights owing to the severe pain chiefly in the shoulder and down the arm, extending into the fingers, with tingling sensations and numbness. He could hardly speak above a whisper, and there was continual stridor from the narrowing of the tracheal tube; he was almost aphonic. He could not lie flat, but had his head constantly elevated, and inclined towards the affected

side. He was becoming gradually emaciated from his want of sleep, and from the constancy of the pain. The aneurismal tumour propelled from behind the sternal end of the right clavicle, and pushed over the attachment of the sterno-mastoid muscle to the other side; it was the size of a moderate orange, felt firm, and pulsated vehemently, with a marked centrifugal impulse. The right jugular vein was considerably enlarged and prominent; the part of the sternum corresponding to the upper portion of the artery, was dull on percussion and about two inches broad; a well-marked bruit was heard over the course of the artery, and along the carotid and subclavian trunks. All the evidences pointed to the case being one of an aneurism of the upper portion of the arteria innominata.

The case was one of great and increasing gravity, the pain along the arm was extremely distressing, and the patient obtained most relief by keeping the arm raised on a pillow, and the forearm thrown over the top of his head; the fits of dyspnœa were also becoming very urgent. I determined on trying the effects of cold combined with the internal use of the persulphate of iron and digitalis.

The patient was kept as quiet as it was possible, the arm was put in the most convenient and easy position, and very well elevated; an ice-bag was kept constantly to the tumour, which gave him so much relief that he had it applied very assiduously.

Fifteen drops of the persulphate of iron liquor with three drops of the tincture of digitalis were given three or four times daily; a meat diet, and an anodyne at night was ordered.

On August 13th, the tumour was apparently increased in size and thinness, especially to the inner side of the sterno-mastoid muscle; in a few days, however, I was much relieved by finding that it was decidedly firmer and less pulsating, and this condition increased from day to day, till finally he was able to be allowed out of bed and take very gentle exercise. The persulphate of iron was now continued, and the cold gradually given up.

He left the hospital on August 11, 1869, apparently with a reasonable prospect of consolidation.

Since his discharge from hospital he returned to the country, and recovered so far as to be able again to resume his occupation as fisherman, chiefly seine fishing. Dr. Gelston, of Limerick, has reported twice within the last two months that the cure is apparently complete, that the tumour is quite consolidated, that there is still some hoarseness, but a total absence of pulsation or bruit.

ART. XIII.—*A Case of Cyclopian Monster.* By WILLIAM ROE, M.D., F.R.C.S.I.; Assistant-Master Coombe Lying-in Hospital.[a]

THE case I have to lay before the Society is one of interest both to the physiologist and the practical obstetrician. I wish to exhibit a specimen of that variety of monstrosity named "cyclops." It will be seen to be a female child of average size and weight, well formed in every part except the head and face, which I will just now describe more fully.

CASE.—On yesterday morning (Dec. 16th), I was called to see a woman, aged thirty-five years, in her fifth confinement, her last child being now only twelve months old. She never has nursed her children. The history I got was that on that morning she was (as usual) getting her husband's breakfast, when she felt a sudden gush of water. She immediately went to bed, and sent to the Coombe Hospital for assistance. A pupil (Mr. Hunt) went immediately, and on making an examination found the placenta and funis in the vagina. On ascertaining this much he sent for me, and I visited her very soon afterwards. I found the placenta completely detached, the left shoulder presenting, and the uterus firmly contracted on the child. I endeavoured to pass my hand into the uterus, but found so much difficulty that I considered it dangerous to persevere. In consultation with my colleague, Dr. Ringland, we agreed that evisceration was a safer course, and accordingly made arrangements for the operation, but on examining the abdomen, which was very large, with the stethoscope, we heard a loud and very distinct *bruit* over the entire anterior surface of the abdominal tumour. We now abandoned the idea of eviscerating, fearing that there might be twins, and perhaps the living one presenting. We again had recourse to version, which was accomplished with very great difficulty, and I delivered her of the monster which I now show the Society.

There was not, however, another child in the uterus, but a large intramural fibroid occupied the anterior wall, and, of course, accounts for what we both heard and felt. The appearances presented by this monster were peculiar in many respects. As I have already stated, the body was that of a well-formed mature child. The head was hydrocephalic, and contained about a pint and a half of sanguino-serous fluid, and the bones on the right side were undeveloped, and there was no roof to the orbit. The brain was very

[a] Read before the Dublin Obstetrical Society.

small, the cavity of the cranium being chiefly occupied by fluid. The cerebellum and spinal cord were normal, and very large in proportion to brain.

Face.—Ocular aperture, diamond-shaped, situated in centre of face, filled up by the eye and two large folds of mucous membrane, one on the outside, the other below and to the inner side, each containing rudimentary bone or cartilage.

Ears.—Normal; but in front of each there was a teat-like appendage.

Mouth and tongue quite perfect.

No olfactory bulbs.

Optic and olfactory nerves could not be found.

The other cranial nerves appeared normal. To my friend Mr. Wilson I am indebted for the description of the eye, which he kindly examined for me.

Eye measurement.—From cornea to posterior pole, 13 milimetres; one lateral diameter, 13 m., while the other measured only 10 m. Cornea opaque, 5 m. from side to side. The sclerotic becomes funnel-shaped where the optic nerve enters the globe.

On making a section of the eyeball, which had been in Müller's hardening solution only for a fortnight, the vitreous humour was found of normal consistency; the crystalline lens, translucent, globular in front, measured at the equator, was 4 milimetres, and from before backwards nearly 5; had some black pigment adherent to its front, and was firmly embedded and adherent to membranous structures posteriorly, from which it was impossible to separate it without violence to the parts; posteriorly, where it was torn from its bed of capsule, it presents an irregular, rough, and elevated surface, showing apparently that lens substance was being deposited at this place. Immediately behind the lens, and intimately adherent to it, is what must be regarded as the posterior capsule, which, together with the retina and choroid, forms a mass of structure inseparably connected to the sclerotic, near the position of the insertion of one of the oblique muscles. At this place the sclerotic presents internally a ridge-like prominence, with which the retina and choroid are united. There are also indications of the hyaloid artery remaining, passing backwards from the posterior capsule to the optic papilla. The retina appears normal, but very thick; in some places loose from choroid, and, except at the place already referred to, easily separated throughout; some few spots of pigment were observed on its internal surface, and pigment remained abundantly

attached to its external surface. The choroid was not fully developed, and was here and there perfectly devoid of pigment; the ciliary processes were just barely discernible, and the pupillary opening of the iris was filled by the membrana pupillaris.

This eyeball may be regarded as having been arrested in its development. The condition of the cornea, the presence of the membrana pupillaris and hyaloid artery, the small and imperfect state of the crystalline lens, the state of the posterior capsule, the incomplete choroid, as well as the general size of the globe; all these are conditions found in the fœtus of about five or six months.

There are several points of practical interest about this case—1st, the presentation; 2nd, was it a case of placenta prævia? 3rd, the choice of operations. 1. Shoulder presentations are said to be very rare in cases of monstrosity, the deformed part usually presenting—thus, when the genitals are malformed, the breech usually presents. We also find in cases of hydrocephalus that the head is the part first reached. 2. I believe, from the fact of the gush of water and the absence of hæmorrhage, that it was not a case of placenta prævia, although the placenta must have had a very low attachment. 3. The most important point, however, was to decide between the two operations which were open to us—evisceration or version; and I have no hesitation in saying that we acted more prudently by performing version, where there was any doubt as to the existence of a second child, which doubt I think we were justified in entertaining. If, on the other hand, we were sure there was but one child, evisceration would have been the safer for the mother, and would have placed her in a better position to make a good recovery, which I am happy to say she has done.

Cyclopian or cyclocephalian monsters have attracted much attention from physiologists and teratologists. Dr. Vrolik[a] mentions five varieties of it. In the 1st, the eyes are absent or not externally visible, and the nose is either absent or replaced by a kind of proboscis or snout-shaped member, consisting of little more than skin and attached above the orbit. 2nd, A single orbit in the centre of the forehead, which contains a single eyeball, and above which there is sometimes a proboscis representing the nose. 3rd, The eye appears externally to be single, but is internally double, and with this again the nose may exist in the form of a proboscis. 4th, The two eyeballs are separated, but they lie in

[a] Over den aard en orsprong der Cyclopie. In the Transactions of the Netherlands Institute. Amsterdam, 1836.

one orbit in contact or with only a narrow partition between them, and above them there is a proboscis which, as in the other cases, may be curved either upwards or downwards. 5th, The proboscis approaches more nearly to the form of a natural nose, has an osseous nucleus, and is directed downwards, and the eye above which it is placed is either double or single.

Now comes the question, what are the causes of this monstrosity? Is it an arrest of development? Tiedman considered it depended on the absence of olfactory nerves and a fusion of optic nerves and optic thalami; but Eller, Hannermann, C. F. Wolfe, Vrolik, &c., have proved that the double optic nerve may exist with a single eyeball, or that with two eyeballs we may only find one optic nerve. They have also seen cases where both eyeballs existed, although no trace of optic nerves were found. Tiedman's supposition, therefore, that these, like other malformations of organs, depend upon a precedent defect in the corresponding nerves, or parts of the nervous centres, cannot be maintained. In favour of the view held by some that it is an arrest of development, the evidence is not so clear. Certainly, as far as the appearance of the brain is concerned, it accords with the early fœtal condition of the organ; but as to the eye it is not so certain that its singleness is the result of arrested development. Huschke's opinion, that the eyes are formed by a single vesicle, which afterwards separates into two, has, I think, been thoroughly controverted by Bischoff, who states that the optic lobes are from the beginning separated and double, taking their origin from the anterior cerebral vesicle, and that from an abnormal condition of this last, by which the rudiments of the eyes approach each other and fuse together, may be derived the cause of cyclopia. M. Vrolik, who also takes this view, appears to think that cyclopia appertains to the Verschmelzungs Bildungen of Meckel in an early period of development; or perhaps it may be compared to the metamorphosis of the eyes of the Daphnia, Cypris, Polyphemus, &c., in which there are originally two vesicular bulbs which subsequently coalesce into one; but, however this may be, it remains certain that the malformation of the facial and cranial bones is the consequence of the abnormal condition of the brain, and of the visual and olfactory apparatus.

ART. XIV.—*Report on Poisonous and Adulterated Confectionery.* By CHARLES A. CAMERON, Ph.D., M.D., L.K.Q.C.P., Professor of Hygiene in the Royal College of Surgeons; Professor of Chemistry in Dr. Steevens' Hospital Medical College; Lecturer on Chemistry in the Ledwich School of Medicine; Analyst to the City of Dublin, &c.

DURING the latter part of the year 1870 I made an examination of a large number of specimens of sugar confectionery on sale in Dublin. The specimens were obtained in the following manner:—An officer of the Public Health Committee of the Corporation of Dublin purchased a quantity of confections at thirteen establishments, wherein the sugar boiling industry is carried on. When buying the samples he gave notice to the vendors of his intention to convey them at once to the city analyst, in order that they might be analysed. This notice was given in accordance with one of the provisions of the Food Adulteration Prevention Act of 1860, which is intended to give the vendor an opportunity of accompanying the purchaser to the analyst, so as to prevent the articles bought from being tampered with *in transitu*.

The total number of different kinds of confections submitted to me for analysis amounted to 123. Those manufactured at three establishments were quite pure: the collections obtained at the other shops (10 in number) contained poisonous pigments and other impurities in a large proportion of the items; and these I shall now describe.

Yellow Confections.—Out of 40 confections coloured yellow, only 2 owed their hue to saffron. 1 was coloured with gamboge; and all the others were coated with chromate of lead, or blumbic chromate, commonly termed chrome yellow. The amount of the latter pigment varied from $\frac{1}{400}$th to less than $\frac{1}{1000}$dth of the weight of the confections. The common sugarstick, sold at 1½d. per two ounces, contained the largest proportion of chromate of lead.

Orange Confections.—12 articles—chiefly lozenges and "sugar almonds"—had a bright orange hue, due to the presence of a variety of chromate of lead.

Red Confections.—38 of the specimens—comprising "peaches" sugar almonds, lozenges, comfits, sugarstick, sugar balls, &c.—had various shades of red, from a faint pink to a bright scarlet. Of

these, 36 specimens were coloured with cochineal, 2 contained vermillion (mercuric sulphide, or bisulphide of mercury) in the proportion of 4 grains per ounce of the confection—which was the cheapest kind of sugarstick.

Green Confections.—At one time arsenite of copper was frequently used to impart a brilliant green colour to confectionery; but the numerous accidents which occurred from the employment of this poisonous pigment have so alarmed the public that green confectionery is now scarcely to be met with. Only 1 of the specimens examined by me was coloured green, and that was only a small figure of a baby, with a green frock on it. The colour was composed of a mixture of Prussian blue and chromate of lead.

Blue Confectionery.—Blue is not a popular colour; only 1 specimen having streaks of this colour was contained in the 123 samples. The pigment employed was ultramarine.

Mauve and Magenta Confectionery.—9 specimens were brilliantly coloured with mauve, magenta, &c. The new dyes, when allowed to come in contact with the skin, occasionally produce toxic effects. Several cases of dermatitis, produced by the use of shirts and stockings coloured with these dyes are recorded in the *Journal of Cutaneous Medicine* for July and October, 1869. Similar cases are described in the *Bulletin de l'Academie Imperiale de Medecine* for February and March, 1869. My friend Mr. Mervyn Crofton, Chief of the Pipe Water Department of the Corporation of Dublin, lately suffered from a severe cutaneous eruption after wearing for a few days a shirt coloured with a magenta dye. The garment was then well washed and boiled for a considerable time, but its toxic power survived these processes. Striped white and Magenta stockings worn by the same gentleman produced inflamed circles on his legs, corresponding in size to the coloured bands on the stockings, proving clearly that the dermatitis was not produced by friction from the stockings.

The Coal-tar Dyes.—As the coal-tar dyes are liable to contain traces of lead, mercury, and arsenic, and as their use for the purpose of colouring confectionery has been prohibited by the authorities in Paris, it would appear to be the safer plan not to place these dyes on any substance intended for human food. The quantity used is, however, so small, that no serious consequences are likely to arise from eating confections coloured with these substances.

An article of food containing more than a grain of chromate of lead per ounce may well be regarded as a slow poison. Soft water containing less than 1 grain of lead per gallon (70,000 grains weight) has often produced poisonous effects on individuals and families. Chromium (an ingredient of chromate of lead) is also a poisonous metal. 6 grains of a salt of this metal injected into the jugular vein of a dog caused the death of the animal. Workmen engaged in the preparation of chromate of potash (potassic anhydro-chromate) often suffer from an ulceration of the throat, resembling that of secondary syphilis; and also from slow necrosis of the nasal bones. I have not the slightest doubt but that the use of confectionery coloured with chromate of lead produces a large amount of infantile disease.

Vermillion was at one time an officinal medicament; but it is no longer administered internally, though still employed in mercurial fumigations. It constituted the basis of Boerhaave's red pill—a celebrated nostrum of former days. There is a general belief in the inertness of this salt of mercury, but I find that it is capable of producing mercurialism, when given in very large doses. In 1½d. worth (2 oz.) of sugarstick I found 8 grains of this salt. As the dose of calomel—one of the mildest compounds of mercury—for a young child is only a grain, it is evident that vermillion, notwithstanding its great insolubility and comparative inertness, if consumed by a child at the rate of even 2 or 3 grains per diem, would produce some ill result. My friend Dr. Benjamin F. M'Dowell has kindly tried the effects of vermillion on some of his patients at the Lock Hospital, and the following are results arrived at:—

"I have employed the persulphide of mercury, or 'vermillion,' in a number of cases since November last, with a view to ascertain whether or not it is 'inert,' and also to discover if any special therapeutic action might be assigned to it.

"In all the cases selected mercurial treatment was indicated.

"In several of them I was not able to pursue my observations sufficiently long to form an opinion as to its action in continued doses. I give a sketch of the particulars of three which are carefully recorded. In small single doses it produces no apparent effect.

"On the 30th November, having first ordered an aperient for each, I directed three of my patients this drug in doses of four grains three times a day, with sugar.

"2nd December.—None of the patients complained of any unusual symptom or sensation, except head-ache, but as this is a manifestation which often follows the use of other mercurial preparations, I attached no importance to it; accordingly they were all directed to have double the previous dose, that is, twenty-four grains per diem.

"8th December.—One of the patients left the hospital, and so I could not pursue the inquiry any further in her case—but neither she nor the other two complained of any abnormal sensations, except slight head-ache and griping pains in the abdomen—in one of the cases there was, I thought, slight mercurial fœtor from the breath—but it was badly defined.

"14th December.—Gums are vascular, evidently tender in one case, and there is a faint mercurial fœtor from the breath; in the other, who is pregnant, there is no mercurial fœtor, and the gums are not at all tender—the latter, however, are somewhat vascularized.

"December 14th to 26th.—There was a gradual development of symptoms of mercurial saturation in the case of the pregnant woman, less marked than in the other case.

"26th.—The medicine was discontinued in each case.

"January 10th.—The gums of each of the patients are still tender; this is remarkable, but there has been no salivation in either case, either during the use of the drug or since.

"16th January—Gums still very tender, yielding blood at the slightest touch. The characteristic blue hue is very apparent. The vermillion used was examined by Dr. Cameron and found to be pure. The patients to whom the salt was administered were visited by him.

"*Remarks.*—The case in which the medicine was discontinued on the 8th December had taken about one hundred and eighty grains without any prominent symptom; the other two took in all about six hundred grains each—in both these cases the usual symptoms produced by a mercurial course manifested themselves; in the one case no doubt less prominently than in the other for for obvious reasons.

"The history of the treatment of these cases clearly proves that the usual symptoms produced by the other preparations of mercury in repeated doses will follow the use of this drug.[a] I am not

[a] The after-effects of the salt appeared to be more decided than those produced by the ordinary merculiar compounds; it would seem as if it were longer retained in the system.

inclined to attach any special importance to it as a mercurial agent, on the contrary, I believe it to be slow and uncertain in its action.

"I have used it in the form of fumigation in putrid ulceration of the throat some years ago in the Lock Hospital, but I found it difficult to regulate its action, and it produced irritation of the air passages. The application of a simple solution of sulphurous acid, or the spray into which it is converted, is better, and other mercurials are more convenient where fumigation treatment is called for.

"Regarded in a hygienic point of view, I believe the continued use of small doses would act injuriously on the constitution.

"17th January, 1871." "B. F. M'Dowell.

Vermillion is very liable to be adulterated. The Chinese vermillion is generally very pure; but cheaper varieties very frequently contain minium, or red lead—an oxide of lead. In the vermillion used to colour the confectionery which came into my possession, I found nearly 20 per cent. of red lead—a far more active poison than mercuric sulphide.

Adulteration of Confectionery.—The lozenges purchased at two of the thirteen sugar confectioners' shops contained from 12 to 15 per cent. of an insoluble white clay, known in the trade under the term of *terra alba.* The peaches, sugar almonds, lozenges, and comfits, contained rice, starch, and gum arabic. Cough lozenges and bath pipe contained gum, sugar, and extract of liquorice—a few of them being slightly medicated by the addition of opium and camphor (probably in the form of paregoric elixir). Small quantities (under 3 per cent.) of plaster of Paris were found in the bath pipe and cough lozenges; but they were probably derived from adulterated liquorice extract.

A figure of a baby in its cradle had the following composition:—The cradle was composed of a mixture of plaster of Paris (calcic sulphate) and sugar; the body of the baby was sugar and rice starch. Its eyes were Prussian blue, its cheeks tinted with cochineal, and its dress was painted with chromate of lead.

The ten sugar confectioners above mentioned, whose wares I have now described, were prosecuted before the police magistrates. Five of them on paying costs (£3) and promising to abandon the use of poisonous pigments, were "let off with a caution;" the others were fined respectively, £5, and £3 costs; £1, and £1 costs; £1, and £1 costs; 10s., and £3 costs; and 10s.

In analysing confectionery for the purposes of a prosecution the methods of procedure must be very accurate. One may, however, readily determine the presence of impurities in the following manner:—Dissolve a little of the article in water; if vermillion be present it rapidly sinks to the bottom of the vessel, whilst cochineal remains in solution. Add a few drops of solution of chloride of lime and the red colour instantly vanishes. Chromate of lead forms an opaque colour, whilst saffron is more or less transparent. Rub off the colouring matter, and heat it to redness in any convenient crucible or capsule. A dirty green or yellowish green residue indicates that chromate of lead had been used. To be certain of this point, dissolve the residue in a few drops of pure nitric acid, heat gently to drive off the excess of acid, add a little water, and test for lead. Solution of ammonic sulphide (hydrosulphate of ammonia) gives a dark brown precipitate, and solution of potassic iodide a bright yellow precipitate. These operations may be conducted on the most minute scale. The coal-tar colours are so remarkable that the eye can hardly fail to recognize them.

Incinerate in any convenient vessel—an iron spoon over a coke fire, if no better apparatus be available—100 grains of the confection. If pure no residue should be obtained. Terra alba remains as a greyish heavy powder.

In conclusion, I think it would be desirable to limit the colours used by confectioners to three, namely—cochineal (or carmine); saffron; and, for opaque yellows, Madras turmeric.

ART. XV.—*Clinical Report of the Rotunda Lying-in Hospital, for the year ending 5th November*, 1870. By GEORGE JOHNSTON, M.D.; Fellow of the King and Queen's College of Physicians; Master of the Hospital, &c., &c.

I CONSIDER that I cannot serve the interests or carry out the great object of this Society (which has always been that of eliciting truth from practical observations) in a more beneficial manner than by giving you a continuation of the clinical reports of the Rotunda Lying-in Hospital, more particularly as I am convinced that the obstetric statistics of large internal maternities, such as it, are the only ones that can be relied upon, and that it is from them only

that a fair conclusion as to the advantages and value of such institutions, with the comforts they afford, can be arrived at.

I am the more urged to do so, in order to bring forward still further proof of the correctness of the remarks I made on a former occasion, viz.—

1st. That zymotic disease (puerperal fever) does not prevail endemically in large lying-in hospitals.

2nd. That puerperal fever, when it is epidemic, does not necessarily make its appearance in great maternity hospitals in the first instance.

3rd. That, where perfect clealiness in every particular is maintained, and labour not allowed to proceed too far unaided, there is no miasm emanating from the puerperal patient.

4th. And that where there is sufficient cubic space of air for each individual, and thorough ventilation observed, the wards can be kept without an hospital atmosphere.

During the past year—that is, from November 6, 1869, to November 5, 1870, inclusive—1,087 deliveries took place in the hospital (344 of which were primipara, 743 were pluripara):—

Of these 851 were natural deliveries; 30 abortion; 11 were tedious—*i.e.*, lasting above twenty-four hours, but where delivery was effected by the natural efforts; the delay in all being in the first stage.

There were 56 preternatural cases—viz., 9 where the upper extremity was the presenting part; 47 where the lower extremity or breech presented; in 19 cases version had to be performed.

There were 86 cases of difficult labour—viz., 83 where delivery was effected by the forceps; 2 had to be delivered by craniotomy; 1 where evisceration had to be resorted to.

There were 84 instances of complicated labour—18 being twins; 2 were triplets; 11 were cases of accidental hæmorrhage; 7 unavoidable hæmorrhage; 11 *post-partum* hæmorrhage; 5 where the placenta was morbidly adherent; 12 prolapse of funis; 5 cases of convulsions; 1 rupture of the uterus; 2 where the perinæum had to be incised; 9 mania; 1 Cæsarean section.

Chloroform was used in 88 cases.

Thus showing that the usual amount of difficulties and dangers had to be contended against, and therefore must be taken into consideration; at the same time it should be borne in mind, that the greater number of poor creatures who seek the shelter of the Institution are persons steeped in poverty and wretchedness, many

of them labouring under great distress and anxiety of mind, and not a few who, having been maltreated in their own wretched rooms by the unskilfulness of their attendants, are at length brought to the hospital, often in such a hopelessly dying state that they pass off in from a few minutes to a few hours after admission.

In order, therefore, that a fair and just conclusion may be arrived at, I purpose to adopt the same plan that I did in my first report, *i.e.*, that of giving a list of the deaths from all causes in chronological order; then taking from it those that died of zymotic disease, and relate the circumstances of each case as it occurred.

Now, by looking at Table No. 1 (p. 158), it will be perceived that the total number of deaths amounted to seventeen—viz., 2 from gangrene of the uterus; 1 rupture; 1 exhaustion from *post-partum* hæmorrhage; 2 from placenta prævia; 1 from accidental hæmorrhage; 3 from peritonitis; 3 from pyæmia; 1 from sloughing of cervix uteri; 2 from pleuritis and pneumonia; 1 from apoplectic convulsions. 6 of these, as you may perceive, were from metria.

The first was the case of a young woman, aged twenty-five (her first pregnancy), whose husband being abroad, and not having heard from him for some months, caused her to fret greatly; and when the period of her time was approaching, her anxiety was not diminished by being (falsely) told, to use her own words, that "*they were dying like rotten sheep in the hospital.*" However, as her means were not sufficient to enable her to be confined at home, she was reluctantly compelled to come in, when, after an easy labour of six hours—second stage lasting only three-quarters of an hour—she was confined of a girl; placenta expelled in half-an-hour; but the state of nervousness she had been brought to, both by the anxiety about her husband and the dread she had of the hospital, had such an effect that, on the evening of the third day, symptoms of peritonitis set in of a low form; she was fomented with turpentine, and given turpentine and opium and aconite by the mouth. Though supported by beef-tea, chicken broth, and wine, and, by the advice of Dr. H. Kennedy, who kindly came to see her, she was given barm, but all without avail; died on eighth day of attack.

The second was a young creature (a lady sent up from the country, having been seduced), aged twenty-four, her first pregnancy. She was admitted in a state of the greatest nervous excitement; in fact, as we ascertained afterwards, she had to be watched closely, lest she might commit suicide, which she had attempted

TABLE No. 1.—*Deaths from all Causes, from Nov. 6th, 1869, to Nov. 5th, 1870.*

No.	Date, 1869	Ward	Bed	Patient		Cause	Observations
				Age	No. of Pregnancy		
1	Nov. 11	3	25	25	1	Peritonitis	Fretting greatly; husband at sea; frightened by being brought to hospital; was told "*they were dying in it.*"
2	„ 22	4	42	29	5	Carcinoma and Gangrene of Uterus	Died 29 hours after delivery.
3	Dec. 11	2	Back Ward	28	3	Rupture Uterus	Cold, anæmic and pulseless on admission; child found in abdomen; delivered by version; died in 15 hours.
4	„ 25	7	Back Ward	35	7	Exhaustion	PPH.—Cold and pulseless on admission; died in 17 hours.
	1870						
5	Jan. 6	2	Back Ward	27	3	Gangrene of Uterus	Delivered by craniotomy; child hydrocephalic and fearfully putrid; discharge for 3 days before most fœtid; died 2nd day.
6	„ 12	12	116	24	1	Peritonitis	Admitted in feverish state; seduction; remorse; attempted suicide before admission.
7	Mar. 13	4	40	23	1	Pyæmia	Most unhappy; drunken husband; child hydrocephalic; DP. very fœtid discharge on admission; pyæmia showed itself immediately.
8	„ 16	3	25	33	1	Sloughing of Uterus	Waters evacuated early in first stage; head resting on cervix for 5 hours; delivered with forceps; died on 4th day; P. M. examination; complete separation of body from cervix by sloughing.
9	„ 26	5	Couch	35	6	Placenta prævia	Admitted in a state of great exhaustion; delivered by feet; sank in 2 hours after.
10	„ 27	3	23	20	1	Pleuritis with Pneumonia	Admitted with intense dyspnœa; easy labour; died fourth day.
11	April 6	3	Back Ward	25	1	Apoplectic convulsions	Died in 13 hours after delivery.
12	May 15	12	110	30	1	Pyæmia	Livid patch of inflammation on posterior part of right labia before delivery, which sloughed; patch appeared on middle finger of left hand and back of right ditto.
13	June 30	4	40	33	4	Placenta prævia	Great exhaustion on admission; died in 2 hours after delivery.
14	July 8	2	Back Ward	30	7	Pleuritic effusion	Moribund on admission; Cæsarean section.
15	Aug. 15	2	17	25	2	Accidental hæmorrhage	Brought in in a state of great exhaustion from accidental hæmorrhage.
16	Sept. 16	5	44	24	4	Peritonitis	Symptoms appeared immediately; had been in great penury and mental anxiety, her husband being in gaol.
17	Oct. 13	8	86	20	1	Pyæmia	A case of seduction; great mental anxiety from time of admission.

previous to leaving home. On admission the report is—her tongue was loaded, white fur; in a state of extreme delicacy; face pallid and anxious; her labour was tedious, being six and a half hours in the second stage, owing to inertia. Two stimulating enemata failing, she was delivered with the forceps of a boy, $6\frac{1}{2}$ lbs., when a second was found in utero; she was given a dose of ergot, with wine; the membranes ruptured, when the head soon descended, and in twenty minutes a girl, $4\frac{3}{4}$ lbs., was born by the natural efforts; placenta soon followed, after which an opiate was given to allay excitement. Peritoneal symptoms showed themselves almost immediately. She was treated by turpentine fomentations; turpentine and opium by mouth and enemata; strength supported with beef-tea, chicken broth, wine, and brandy. She died on the sixth day of the attack.

The third (a case of pyæmia) aged twenty-three; first pregnancy; a small delicate-looking woman, with hectic flush, ill-nourished, thin, and emaciated; married ten months; since which she never had a happy moment, from the ill-treatment of a dissipated husband, who (in a drunken fit) three weeks before her admission, attempted to throw himself out of a window, when, by the exertion she underwent in restraining him, and the fright consequent upon it, she ceased to feel the child. Her labour was slow in the first stage. Discharge, very fœtid. She gave birth to a girl, extremely putrid, after a labour of twenty hours, second stage lasting only one hour. The lochia were very fœtid. Ordered vagina to be syringed with solution of condy, strength supported. Abdomen became tympanitic, tender, hectic flush increased, perspiration, and all the symptoms of low hectic, pyæmic patches appearing on elbow, wrist, &c.; put on bark with chlorate of potash. Every means were adopted to support her strength; but she eventually died on the tenth day after confinement

The fourth (a case of pyæmia) aged thirty; her first pregnancy; a worn, delicate-looking woman, feverish, with constant thirst. When examining her, a livid patch was observed, on the posterior part of right labium. Her labour was tedious in the second stage, being six and a half hours, owing to inertia and rigidity of the soft parts. Two stimulating enemata producing no effect, she was put under the influence of chloroform, and delivered, with the forceps, of a boy, living. The livid patch, before observed, soon merged into an extensive slough. Her strength was supported by bark, beef-tea, and wine. Slough poulticed with charcoal. On second day abdomen became tympanitic. Ordered fotus terebinth. and poultice,

and draughts of turpentine, acet. opii and tinct. aconiti. On fifth day a pyæmic patch appeared on the middle finger of left hand, and on the following day another patch appeared on the back of the right hand. She died on the seventh day after delivery.

The fifth (a case of peritonitis), aged twenty-four, her fourth pregnancy; was confined after a natural labour of twelve hours' duration, the second stage lasting three hours, giving birth to a girl, living. On the day after delivery found her fretting and anxious. Abdomen tympanitic and tender; tongue dry, with brown fur; pulse, 125. Ordered, at once, fotus terebinth and cataplasm lini, haust terebinth, cum acet. opii, every four hours; full anodyne at night; strength supported with beef-tea, wine, brandy, and champagne, the latter of which had to be stopped in consequence of producing flatulency. At times she showed symptoms of improvement, and we had hopes of her; but she eventually sank on the eleventh day. Ascertained that she had been in the greatest penury and want for the last two months, and fretting greatly, her husband being in gaol.

The sixth (a case of pyæmia), aged twenty, her first pregnancy; admitted in a state of great mental anxiety and excitement. Tongue brown furred; pulse, 110; complaining of pain in the right iliac region; ascertained that she had been *seduced*. Her labour was tedious, owing to her feverish state, &c. Had to be delivered with the forceps, the head having made no advance for nearly four hours—the child, a girl, 6 lb. 7 oz., living. Placenta was morbidly adherent, and had to be removed. Patches of pyæmia appeared on both upper and lower extremities shortly after delivery. Strength was supported in the usual way. Put on bark with chlorat. of potass.; but all to no purpose. She lingered to the tenth day.

I may mention here that in no one instance of the foregoing did the sickness spread to any patient either in the ward or hospital; and while this perfect exemption from epidemic sickness existed within the walls, epidemics from time to time prevailed outside, and that to a very great extent.

The remainder (eleven deaths), which I may include under the category of accidental, were as follows:—Two from gangrene of the uterus, one being a woman 29 years of age, her fifth pregnancy. Her history is as follows:—On admission, she complained of great abdominal pain, which prevented her sleeping for the last three nights; extremely tender on pressure over uterus, particularly in the left hypogastric region; has had difficult labour in her previous confinements, the two first having to be delivered by craniotomy;

her third (a girl, very small,) was delivered by the natural efforts, and alive. On examination the os was found to be the size of a two-shilling piece, hard, cartilaginous, and jagged. Could not satisfactorily examine it, in consequence of giving her extreme pain, attended with hæmorrhage; discharge very offensive. The first stage lasted twelve hours, the head being in the cavity, where it remained without any advance, notwithstanding strong pains for one hour. The bones of the head overlapping, and *caput succedaneum* forming, she was put under chloroform, and delivery effected with the forceps at two a.m. Discharge very fœtid; to be syringed with Condy's solution. Eight and a half hours after complained of sickness of stomach, when she was ordered a draught containing acid hydrocyan., sodæ carb., sol. m. morphiæ, æth. chlr., aq. cinamomi. 6 p.m.—Abdomen tympanitic with great tenderness; pulse 100; ordered fotus terebinth et cataplasm lini, and full anodyne; beef-tea. To be syringed as before.

22nd—At six a.m., found in a state of collapse; died at seven a.m., twenty-nine hours after delivery. Husband would not allow a *post-mortem* examination.

The second was a woman, aged twenty-seven; third pregnancy; admitted into chronic ward four days before confinement, with extremely offensive discharge; abdomen very tender on pressure. Had to be delivered by craniotomy in consequence of the hydrocephalic state of the head, owing to putridity. Being very weak, she was ordered some beef-tea and wine. 6 p.m.—Abdomen large and tender; ordered fotus terebinth, and poultice; extract opii gr. i. every three hours; brandy, ʒii.

5th.—Pulse, 132; temperature, 102° 3·88″. Repeat pill as before. Beef-tea, wine. 6 p.m.—Pulse, 140; temperature, 103° 1·15″; respiration, 40. Coffee ground vomiting; ordered enema of beef-tea, with spirits ammonia aromat; tinct. opii. To have brandy and ice. 11 p.m.—Repeat enema; brandy and ice.

6th.—1 a.m.—Died. On a *post-mortem* examination, the whole uterus was found in a state of sphacelus.

The third was a case of ruptured uteri; aged twenty-eight; third pregnancy; was brought into hospital cold and pulseless, complaining of great abdominal pain, referred to fundus uteri; on examination found the child in the abdominal cavity; was delivered by version; died in 15 hours. *Post-mortem* showed a very extensive rent at the posterior part of the cervix.

The next was a case of *post-partum* hæmorrhage, having given

birth to her baby at home; was brought in cold and pulseless, in fact, in a dying state; died in 17 hours.

The fifth was a case of complete separation of the body of the uterus from the cervix by sloughing. In this instance the liquor amnii had escaped at the commencement of labour; the first stage was slow in consequence, the cervix becoming thin and expanded on the head, which had descended low down into the pelvis. The warm bath was used, when the os gradually dilated to the size of a six-shilling piece; when, after waiting for five hours without any advance, notwithstanding two stimulating enemata having been administered, the forceps were applied, and a girl 6 lbs. weight was delivered. In twelve hours symptoms of inflammation presented themselves; abdomen became tympanitic and tender; the usual treatment, such as turpentine fomentations and poultice, turpentine draughts and anodyne; vagina syringed with solution of condy; wine, beef-tea, &c. On the third day she fell into a state of collapse, and died in a few hours. *Post-mortem* examination discovered complete separation of the cervix from the body of the uterus by sloughing.

There were two deaths from placenta prævia. The first, aged thirty-five; sixth pregnancy. She was admitted greatly exhausted, having had hæmorrhage for the last month; placenta could be distinctly felt over os, which was about four-fifths dilated; patient, being very restless and intractable, was put under chloroform, and delivery effected by footling; the placenta soon followed, but with severe hæmorrhage, which so exhausted her, that notwithstanding all our efforts to give brandy and beef-tea, which was most difficult, in consequence of the resistance she made to prevent it, she died in two hours. In this instance solution of perchloride of iron was injected, but without avail.

The second, aged thirty-three; fourth pregnancy. Admitted at 11 a.m. in a state of great exhaustion from hæmorrhage; os found size of half-a-crown, placenta covering it; she was at once plugged and strength supported, and given an opiate. At 2 p.m., as we found her weaker, the plug was withdrawn, when the os was found considerably relaxed; the hand was at once introduced by the side of the placenta; membranes ruptured; knee seized and brought down into the vagina; child turned and extracted alive (girl, 7 lbs.); placenta was removed immediately; ergot given; but the draining continued, notwithstanding the injection of the solution of perchloride of iron. We had sent for Dr. M'Donnell to assist us in the operation of transfusion, but as the hæmorrhage continued

till the last, there would have been little use in having recourse to it; in fact she passed away as he arrived at the bed.

Eighth, aged twenty; first pregnancy; admitted with intense dyspnœa, owing to an acute attack of pleuritis with pneumonia, from which she has been suffering for the last seven days without any treatment; os found nearly fully dilated; child, a boy (dead), 8 lbs., was born by the natural efforts in three hours after admission. Her labour in every respect was perfectly natural, but notwithstanding all our efforts to control the disease, she sank on the fourth day after delivery.

Ninth was a case of eclampsy; aged twenty-five; first pregnancy; was admitted comatose, having had several fits before coming in, and recurring every twenty minutes to half an hour; os found size of a shilling. She was at once ordered a fœtid enema; cold affusions to head, sinapism to calves of legs, and chloroform. In about three hours, os being found sufficiently dilated and relaxed, the head in the cavity, the forceps were applied, and delivered of a girl (still-born); she had twenty-two fits; placenta expelled in two minutes. The fits continued as before, notwithstanding sinapism to nape of neck. Bromide of potass. and belladonna by enema, every three hours. She sank in thirteen hours after delivery, having had fifty-three fits.

Tenth, aged thirty; seventh pregnancy; was admitted in a state of intense dyspnœa; pulse hardly to be felt; got her on the bed; found the os not more than the size of a shilling. While making the examination she died. We at once performed Cæsarean section, in hopes of saving the child, which breathed for two or three inspirations, but could not be resuscitated: it was a boy.

The eleventh case, aged twenty-five; second pregnancy; admitted in a state of great exhaustion from accidental hæmorrhage, caused by over-exertion; about seventh month of pregnancy; os high up; could not reach the membranes; placental bruit heard on left side above crest of ilium; binder applied, and given a full dose of ergot with an opiate. This had the effect of sickening her, when the membranes ruptured, waters escaped, after which there was little or no hæmorrhage externally; went on till the 12th, when pains set in, and after a labour of four hours was delivered of a girl, living, 3 lbs. 6 ozs., when a second was discovered; ergot with wine was given, and in a quarter of an hour a girl, living, 3 lbs., was expelled. Placenta was retained for one hour; found morbidly adherent; had to be removed, and with it a large clot;

great *post-partum* hæmorrhage followed, injected with solution of perchloride of iron. Was ordered brandy, beef-tea, and opiate. On the 14th found in a state of collapse; pulse hardly perceptible; abdomen quite soft; uterus well contracted; no tenderness whatever; given bark with chlorat potass., brandy; vagina syringed with Condy's solution—all without effect. She sank at two o'clock a.m. on the 15th.

Post-mortem examination—Uterus found in a state of slough over three-fourths of the surface of its interior; no peritonitis whatever existed.

Number of Patients delivered in each Ward during the Year ending 5th November, 1870.

	No. 1	No. 2	No. 3	No. 4	No. 5	No. 6	No. 7	No. 8	No. 12	Total
November, -	7	9	8	10	8	8	9	7	8	74
December, -	9	8	8	10	13	9	10	8	10	85
January, -	13	12	8	7	8	11	9	9	13	90
February, -	8	7	7	7	8	7	9	9	8	70
March, -	8	12	12	12	13	12	16	13	10	108
April, -	11	12	10	12	8	7	10	9	12	91
May, -	8	7	7	10	11	12	11	9	7	82
June, -	10	10	11	8	6	6	11	11	12	85
July, -	12	8	7	12	11	11	13	13	12	99
August, -	11	11	10	10	10	11	12	10	13	98
September, -	9	12	13	12	11	10	12	7	11	97
October, -	12	13	10	7	8	8	11	8	14	91
November 5, -	—	—	4	4	4	3	1	1	—	17
	118	121	115	121	119	115	134	114	130	1,087

I may now draw your attention to the total number of deliveries that took place in each separate ward during the year, and the number of deaths from zymotic diseases that occurred amongst them.

In No. 1 ward there were 118 deliveries and no deaths.

In No. 2 ward there were 121 deliveries and no deaths of a zymotic nature.

In No. 3 ward there were 115 deliveries and 1 death from peritonitis.

In No. 4 ward there were 121 deliveries and 1 death from pyæmia.

In No. 5 ward there were 119 deliveries and 1 death from peritonitis.

In No. 6 ward there were 115 deliveries; all recovered.

Table No. 2.—*Number of Patients Admitted and Delivered in the Rotunda Lying-in Hospital from each Municipal District, the number of Deaths from Zymotic Diseases that occurred amongst them, and the number of Deaths from the same causes occurring in the different Districts in the same period.*

	North City Districts									South City Districts											
	No. 1			No. 2			No. 3			No. 1			No. 2			No. 3			No. 4		
	Admitted into hospital from No. 1	Died in hospital from zymotic disease	Deaths in No. 1 District from zymotic disease	Admitted into hospital from No. 2	Died in hospital from zymotic disease	Deaths in No. 2 District from zymotic disease	Admitted into hospital from No. 3 District	Died in hospital from zymotic disease	Deaths in No. 3 District from zymotic disease	Admitted into hospital from No. 1 District	Died in hospital from zymotic disease	Deaths in No. 1 District from zymotic disease	Admitted into hospital from No. 2 District	Died in hospital from zymotic disease	Deaths in No. 2 District from zymotic disease	Admitted into hospital from No. 3 District	Died in hospital from zymotic disease	Deaths in No. 3 District from zymotic disease	Admitted into hospital from No. 4 District	Died in hospital from zymotic disease	Deaths in No. 4 District from zymotic disease
From 6th Nov., '69,																					
to 4th Dec., inclusive,	14	1	11	21	—	15	9	—	37	—	—	29	6	—	23	11	—	18	10	—	10
to January 1, -	21	—	7	19	—	10	8	—	11	4	—	24	8	—	9	3	—	13	11	—	9
to February 5, -	20	1	22	30	—	14	6	—	20	4	—	42	13	—	14	6	—	21	16	—	20
to March 5, -	21	—	7	22	—	9	7	—	22	1	—	28	13	—	7	7	—	15	7	—	13
to April 2, -	18	1	7	22	—	9	9	—	10	4	—	20	14	—	15	14	—	12	13	—	22
to May 7, -	26	—	18	36	—	13	6	—	19	2	—	26	10	—	5	7	—	14	18	—	17
to June 4, -	14	—	9	23	—	5	5	—	17	5	—	17	7	—	7	10	—	10	10	1	13
to July 2, -	24	—	10	17	—	7	3	—	6	2	—	20	10	—	9	9	—	6	12	—	8
to August 6, -	24	—	12	32	—	2	5	—	19	4	—	18	10	—	8	12	—	15	16	—	14
to September 3,	24	—	22	19	—	14	5	—	28	5	—	34	9	—	16	13	—	19	13	—	13
to October 1, -	22	—	20	25	—	24	9	—	27	3	—	24	10	—	23	4	—	23	14	1	20
to November 5,	31	—	19	30	—	26	10	—	40	—	—	36	7	—	20	17	—	24	9	1	30
1869–'70, - -	259	3	164	296	—	148	82	—	256	34	—	318	117	—	156	113	—	190	149	3	189

In No. 7 ward there were 134 deliveries and no deaths of a zymotic type.

In No. 8 ward there were 114 deliveries and 1 death from pyæmia.

In No. 12 ward there were 130 deliveries and 1 death from peritonitis and 1 from pyæmia, with an interval of four months and three days between them.

Although we had a comparatively small amount of mortality, still our patients were not completely exempt from sickness; for occasionally instances of puerperal inflammation, such as peritonitis, pyæmia, &c., appeared. I have therefore framed the table, numbered 3, of all the cases of metria which occurred, in chronological order; and as it includes those cases that died, which have been mentioned before, there is no necessity for entering into the details of them again.

I may here say that all the sickness was of an asthenic type, requiring a nutritious diet, such as beef-tea, chicken broth, with the use of stimulants, wine or brandy. The treatment in the peritoneal cases consisted in turpentine fomentations and linseed meal poultices to the abdomen, together with turpentine, from ʒ i. to ʒ ii., with opiates by mouth, but should we find the patient objected to it in that way, it was administered by enema, and where the pulse ran very high and frequent, the tincture of aconite in small doses was added. In some few instances, hydrar. cumcreta with soda siccata and Dover's powder, was given, but we did not rely on it.

In cases of pyæmia, the strength was supported by a generous diet, with wine or brandy, together with chlorate of potass. in infusion of bark; the pyæmic patches were covered with finely powdered starch, which was placed on cotton wadding and oiled silk.

There were 29 cases of peritonitis, all of which recovered, except the three already recorded.

Fifteen of these were primipara; 14 were pluripara.

Among the recoveries was one which I may mention as a remarkable instance of the wonderful influence the mind has over the body in the event of such cases:—

M. B., aged twenty, first pregnancy; labour being tedious in the second stage, was delivered with the forceps; the following day, on morning visit, found her uneasy with head-ache. Pulse 94; tongue clean; abdomen soft, no tenderness. Ordered a draught

containing Hoffman's æther. In the evening in the same state. Ordered an anodyne, with cathartic draught, in the morning.

On the second morning, is fretful, anxious. Pulse 100; abdomen tender; bowels affected twice. Ordered turpentine fomentation and poultices, turpentine draught with acet. op. every three hours; beef-tea and anodyne at night.

On the third day found she had rested badly, still nervous, with anxious expression. Continue the treatment. At evening visit, feeling weak, ordered wine with an anodyne.

On the fourth, pulse 120; temperature 104°; tongue dry, with brown ridge in centre; stomach sick; abdomen tympanitic and tender. Repeat fomentation and poultice; enema, with turpentine and opium, and to take a draught with acid hydrocyanic; sol. mur. morphiæ; soda carb.; æth. chlor. in acq. cinnamon, every three hours; brandy, beef-tea, in small quantities at a time. At noon, pulse 130; same state. Continue the draughts, with tinct. aconite. Ascertained that she was fretting in consequence of her husband having deserted her, and that she was in a most destitute state, having no friends or any where to go. We cheered her up by promising to get her a situation, and that her child should be provided for. At 6 p.m. pulse 120; feels better; no sickness. Continue the beef-tea, wine, and anodyne as last night. On the fifth day, report is, slept well; pulse 76; tongue clean and moist; abdomen soft, no tenderness; complaining only of thirst. Ordered saline mixture with spts. ammonia aromat every fourth hour; beef-tea, wine, as before.

On the sixth, was convalescent, and eventually went out well.

There were five cases of pyæmia, two of which recovered. The first, A. B., aged twenty-six, her second pregnancy, was delivered of a girl, living, which presented by the breech, her labour lasting only five hours.

On the evening of the third day abdomen found tympanitic and tender; pulse 94; tongue white, with red edges. She was ordered the turpentine fomentation and poultice, to take turpentine draught with anodyne. The tympany and tenderness were less on the following day. Repeat the fomentation, poultice, and draughts every fourth hour; to have beef-tea. On visiting her at 6 p.m. of her fifth day, found she was vomiting and purging; abdomen soft, but slight tenderness. She was ordered chalk mixture, with hydrocyanic acid, and an enema of starch, with acetate of lead and opium; wine. At 10 p.m. pulse 120; temperature 101°·4′; still vomiting;

sinapism to the epigastrium, after which a poultice sprinkled with opium to be applied; repeat the draughts and enema; to have brandy, ice, &c. At 1 a.m. (sixth day) diarrhœa again; has vomited; pulse 120; very weak; breathing hurried; sighing; breath cold; extremities icy. Ordered hot applications to her feet, legs, and thighs. Repeat the sinapism to the epigastrium; brandy occasionally, by mouth, or enema; chicken broth; arrow-root. 9·30 a.m., seventh day, pulse 120; very weak; no sleep; sighing constantly; complaining of great prostration; no abdominal pain; sickness still continues; temperature $100\frac{1}{5}^{\circ}$. Repeat the draughts, chicken broth, wine, or brandy, ice; and enemata, with arrow-root, beef-tea, and aromatic spirits of ammonia. 5·30 p.m.—Pulse hardly perceptible; tongue moist; vomiting grumous fluid; diarrhœa continues; extremities cold; complaining of a lump in her neck which prevents her swallowing; to be fomented; hot applications to her extremities; repeat the starch enema as before. At 7 p.m., same state; by the suggestion of Dr. Henry Kennedy, who kindly saw her with me, the strong liquor ammonia was applied over the stomach, and she was given bicarb. soda, gr. xx.; solution mur. morphia, gtts., x.; aq. font. ʒ i. every three or four hours. At 9·45 p.m.—Had some retching, but no vomiting since; no diarrhœa; pulse somewhat improved; complaining of her throat. A small sinapism to be applied; repeat the draughts and enemata.

8th day—Pulse fuller; quieter; aspect improved; breathes easier; can swallow better; extremities warmer; dozed a little; continue the treatment. 6 p.m.—Bowels opened once; pulse 116, fuller; tongue moist; no sickness; temperature of skin natural; repeat the draughts and enemata, &c., as before.

9th day—Slept at intervals; B. O. 3; dark, thin, and very fœtid; pulse 100; no sickness; expression improved; continue the treatment, with chicken broth, brandy, and arrow-root. 6 p.m.—B. O. 1; pulse, 100; no sickness; wine ʒ ii.

10th day—Slept well; B. O. 2; bilious, very fœtid; pulse 88; tongue cleaning, moist; no sickness; repeat the draughts; chicken broth, &c. 6 p.m.—No sickness; B. O. 3; enema amyli, with tincture of catechu and tincture of opium, and repeat in three hours if necessary; complaining of pain in right carotid region; ordered fotus papaver et cataplasm. 11 p.m.—Pulse 120, thumping; temperature 102; pain in part increased; to have a full anodyne, and tincture opium, sprinkled over poultice.

11th day—Pulse 100; slept badly; parotid gland swollen, extremely painful; continue the poultice, with laudanum; to have porter, as she expressed a desire for it. 6 p.m.—B. O. 1; feels easier; tongue moist; swelling increased; no fluctuation; continue treatment.

13th day—Pulse 92; B. O. 2; a purulent discharge from her ear has taken place; put on bark, with chlorate potass.; continue poultice as before; porter.

14th day—Swelling increased to such an extent as to prevent her opening her mouth, and rendering deglutition very difficult; ordered six leeches to be applied to the tumour and poultice; continue the bark, &c.

On the 16th, abscess opened by Mr. Adams, giving exit to a great quantity of purulent matter, after which she gradually recovered, and went out well.

The second case, M. W., aged forty, first pregnancy; innupta; her labour was tedious, owing to the head being in the second position; was obliged to deliver her with the forceps; child, a girl, living; placenta expelled in fifteen minutes.

Eighteen hours after delivery pulse was 100; great tympany of abdomen; ordered fotus terebinth et cataplasm lini, and draught with turpentine and full anodyne.

On the morning of 2nd day pulse found 100; tongue moist; abdomen still tympanitic and tender; continue fomentation and poultice; turpentine draught, with acetum opii, beef-tea. 6 p.m.—Pyæmic patch appeared on back of right hand, to be covered with powdered starch and cotton wadding; ordered to be put on bark with chlorat of potass.; beef-tea, wine, anodyne.

3rd day—Pulse 100; tongue moist; slept; continue the treatment.

5th day, 6 p.m.—Pulse 120; abdomen soft, no tenderness; patch appeared on left elbow and over right trochanter, to be dressed as the hand; continue the treatment, with anodyne.

6th day—Pulse 128; tongue dry and brown; temperature 103° 1; effusion into left knee, very painful, to be dredged with the starch, and covered with cotton wadding and oiled silk; continue the bark with chlorat of potass.; beef-tea, wine, and anodyne at night.

8th day—Patch appeared on outer condyle of left humerus; pulse 100; continue the treatment. 6 p.m.—Some diarrhœa; ordered an astringent mixture, with anodyne.

9th day—Slept; pulse 100; tongue clean and moist; abdomen

soft; patches on hand, elbow, and thigh not extending; knee more swollen, to be dressed with mercurial ointment and poultice; continue the treatment.

10th day—Patches better; great effusion into knee, to be painted over with tincture of iodine, and poultice applied.

11th day—Pulse 100; has not found any benefit from the iodine; requested to have the starch and cotton wadding re-applied. She gradually got better, and went out well, with perfect motion in the knee.

I have thought it fair to mention these two cases as strong proofs of the healthy nature of the air of the hospital.

Now by looking at the Table No. 3 it may be seen that of the thirty-four instances where puerperal inflammation took place, 19 were primipara; 15 were in women who had been delivered before—viz., 4 in their second pregnancy, 4 in their third pregnancy, 4 in their fourth pregnancy, 1 in the fifth, and 1 in the sixth pregnancy.

Twenty-two were delivered by the natural efforts, one of which was the second of twins; 11 were delivered with the forceps, one of which was the first of the above twin case; 2 were lower extremity presentations.

Of the 19 primiparous cases, 14 recovered and 5 died; of the 15 pluripara, 14 recovered and 1 died.

Being still convinced that metria does not exist as an endemic disease in the Rotunda Hospital; on the contrary, that whilst epidemics prevail outside, the hospital itself may remain perfectly free from them, I am induced to lay before you the following table, framed as No. 3, of my last report, in accordance with that of the Registrar-General's, which, although it is no doubt defective, as a commentator has remarked, not giving the "relative population of the districts at different ages and of each sex, the sex and age of those whose deaths are given in the table, how many of the deaths were from the zymotic diseases of child-bed, and the number of births in each district," all of which would render it more satisfactory, still I cannot agree with the same commentator that it is "utterly useless," inasmuch as I conceive that it is quite sufficient to show that sickness of a zymotic nature may prevail, and that to a great extent, in these several districts from where patients come to the hospital, and yet the institution itself be perfectly free from any sickness. It matters not what the age, sex, or state (married or single, old or young) of the victims of these diseases

Table No. 3.—*Puerperal Inflammation; Peritonitis; Pyæmia.*

ate	No. of Ward	No. of Bed	No. of Pregnancy 1st	Subt	Duration of Labour 1st Stage	2nd Stage	Presentation Natural	Pr. Natural	Mode of Delivery Natural	Instrumental	Time of Attack. Hours after Delivery	Result to Mother Recovered	Died	
69,					Hrs.	Hrs.								
ct. 28	3	25	1	—	5½	¾	N.	—	N.	—	50	—	1	Cause—fretting at husband being away at sea and fright.
ov. 12	8	85	—	9	5	3	N.	—	N.	—	48	1	—	
,, 13	8	87	—	6	38	2	N.	—	N.	—	48	1	—	Very delicate, badly fed, came from Walsall.
,, 25	12	114	—	4	2¼	¾	N.	—	N.	—	12	1	—	Weakly constitution.
,, 29	3	23	1	—	14	4	N.	—	N.	—	36	1	—	
,, 30	4	41	1	—	4½	½	N.	—	N.	—	20	1	—	
ec. 12	1	4	—	3	7½	½	N.	—	N.	—	48	1	—	
,, 26	8	87	1	—	18	2	N.	—	—	Forceps	24	1	—	Innupta.
70,														
an. 2	12	116	1	—	11½	6½	N.	—	N.	F.	Immediately	—	1	Twins, excessive mental depression, innupta.
,, 3	12	110	—	3	11½	4½	N.	—	—	F.	30	1	—	
,, 10	4	42	1	—	19	1	N.	—	N.	—	12	1	—	
,, 15	8	87	1	—	13	3	N.	—	—	F.	40	1	—	Great mental depression, husband having deserted her; cheered her up, promising protection; wonderful effect.
,, 24	6	69	1	—	4½	1½	N.	—	N.	—	48	1	—	
,, 31	2	19	—	4	11½	¾	N.	—	N.	—	54	1	—	
eb. 17	4	39	1	—	20	3	N.	—	—	F.	40	1	—	
,, 19	5	46	—	2	4	1	—	Breach	—	—	54	1	—	Pyæmia, choleraic collapse; cynanche parotidœa.
ar. 3	4	40	1	—	19	1	N.	—	N.	—	6	—	1	Pyæmia.
,, 30	5	49	1	—	17	1	N.	—	N.	—	20	1	—	Bronchitis on admission.
pril 4	3	23	1	—	18	5	N.	—	—	F.	9	1	—	Innupta.
,, 14	1	4	—	3	17	2	N.	—	N.	—	Before confinement	1	—	Cellulitis, with peritonitis.
,, 16	3	25	—	3	1	1	N.	—	N.	—	4	1	—	
ay 8	12	110	1	—	11½	6½	N.	—	N.	—	Before delivery	—	1	Pyæmia; livid patch on labium on admission,
,, 16	7	55	1	—	25	—	N.	—	—	F.	30	1	—	Pyæmia, effusion into left knee, innupta.
une 10	6	67	—	2	3	5	—	Foot	—	—	24	1	—	
,, ,,	3	23	1	—	22½	2½	N.	—	N.	—	34	1	—	
uly 4	7	60	1	—	5½	1½	N.	—	N.	—	45	1	—	
,, 11	4	39	1	—	17¼	2¾	N.	—	N.	—	62	1	—	
ug. 29	6	69	—	4	17¼	5¾	N.	—	—	F.	3	1	—	
ept. 5	5	44	—	4	9	3	N.	—	N.	—	20	—	1	Desponding on admission; *husband in prison.*
,, 14	3	25	1	—	20	4	N.	—	—	F.	7	1	—	
,, 20	12	113	—	5	4½	¼	N.	—	N.	—	58	1	—	
,, 22	1	3	—	2	3 hrs.		N.	—	N.	—	48	1	—	Accidental hemorrhage.
ct. 3	8	86	1	—	31½	4½	N.	—	—	F.	On her	—	1	On admission tongue dry and brown, pulse 100, great mental depression; innupta, pyæmia.
,, 18	3	25	—	2	10½	1½	N.	—	—	F.	40	1	—	

may have been. All that is really necessary to show is, that disease of a contagious nature prevails outside, and yet the hospital may be perfectly free from it. I therefore have no hesitation in laying the table before you as one of the great proofs of the correctness of my statement.

Thus, you perceive that from No. 1 North City District 259 patients were admitted and delivered, out of which we had 3 deaths of a zymotic nature, while we find that 164 deaths from zymotic diseases occurred in the district within the year. From No. 2 North City District 296 women were admitted and delivered, without any deaths ; while there were 148 deaths from zymotic diseases in the district in the same period. From No. 3 North City District 8 women were admitted and delivered. All recovered; yet there were 256 deaths from zymotic diseases in the district. I think it well to mention here that one woman was admitted and delivered who came from a house in which scarlatina prevailed. She went through her confinement without a bad symptom, was discharged on the 8th day, quite strong, as were all the other patients in the ward.

From No. 1 South City District, 34 patients were admitted and delivered; all recovered; yet we find 318 deaths from zymotic disease in that district recorded in the Registrar-General's reports.

From No. 2 South City District, 117 patients were admitted and delivered; all recovered; we find by the report that 115 deaths took place in the same district.

From No. 3 South City District, 113 patients were admitted and delivered; all recovered; we find, however, 190 deaths from zymotic disease recorded.

From No. 4 South City District, 149 patients were admitted and delivered, out of which there were 3 deaths; we find that 189 deaths from zymotic disease took place in the district.

With regard to miasm, emanating from the patients, or hospital atmosphere existing in the wards, I have the testimony of numerous visitors, both professional and others, who have all expressed their opinion that neither could be detected; and I may say that, with the exception of a very few cases, we were not obliged to use any means to suppress a fœtid discharge.

Now, as I stated before, comparing the great amount of mortality which prevails in the various districts outside with the small amount of deaths that have taken place in the hospital, and, as I remarked, more particularly, when we take into consideration all the peculiar circumstances and conditions under which the different fatal cases

were admitted, we have no reason to regret keeping the portals of such an invaluable institution as freely open as it ever was intended they should be.

FORCEPS DELIVERIES.

There were 83 cases where we found it necessary to use the forceps; 58 of which were primipara, and 25 multipara; 77 of the mothers recovered. In the application of the forceps we were always influenced by the length of time the patient was in the second stage, never allowing it, in the generality of instances, to exceed 4 hours. We interfered sooner if the pains were strong, and no advance made by the head, or if the liquor amnii had been evacuated at an early period of the labour. When we found there was no advance, say for 2 hours, we usually administered a stimulating enema, then waited for an hour or two, according to circumstances, and if the pains were not producing any effect, a second enema was given, and if this did not succeed, in another hour we gave a dose of ergot (particularly if it was a multiparous case); and if the patient was irritable, we put her under chloroform, and then after passing the catheter, we proceeded to apply the instruments.

Of the 6 fatal cases in this series, the first was aged twenty-nine, fifth pregnancy, suffering under carcinoma uteri and gangrene; died in 30 hours. The second, aged twenty-four, primipara; great distress of mind; attempted suicide; died of peritonitis. The third, aged thirty-three; primipara; there was complete separation by sloughing of the cervix from the body of the uterus; *vide* Case V. of accidental deaths. The fourth, aged twenty-five; primipara; apoplectic convulsions brought in comatose. The fifth, aged thirty; primipara; died from pyæmia, which showed itself previously to delivery. The sixth, aged twenty; primipara; was a case of seduction; labouring under great mental depression; died of pyæmia.

8 children, viz., 3 male and 5 female, were dead born. Of the male—1 was dead and putrid, in the case of gangrene. 1 was in a case where the membranes had ruptured early; the head was arrested for 3 hours. 1, a face presentation; os not completely dilated. Of the female—1 was in the case of convulsions. 1, the membranes had ruptured early; funis rotten. 1, a case of inertia. 1, a case of accidental hæmorrhage. 1, where the mother had condylomata in a sloughing state; had to send her to the Lock Hospital.

The long forceps were used in seven instances, where the head was at or above the brim; all were delivered of boys, and all, both mothers and children, recovered. The first, aged twenty-three, her first pregnancy, the head was arrested at the brim, in consequence of its being in the fourth position; weight of child, 7lbs. 14oz. Second, aged twenty-nine, her fourth pregnancy; the head being in the third position was the cause of its arrest; weight of child, 8lbs. 8oz. Third, aged twenty-two, her first pregnancy; the head was delayed at the brim from disproportion; weight of child, 9lbs. 6oz. Fourth, aged thirty-two, her second pregnancy; the head never entered in consequence of projection of sacral promontory; there was flattening also of its concavity; weight of child, 6lbs. 10oz.; a second child was found and turned, but was obliged to deliver the head with the forceps; weight, 6lbs. 10oz. Fifth, aged twenty-eight, her first pregnancy; funis prolapsed and ceasing to pulsate; head at brim; weight of child, 6lbs. Sixth, aged twenty-eight, her fourth pregnancy; head detained above the brim in consequence of the projection of the sacral promontory; weight of child, 9lbs. Seventh, aged twenty-six, her second pregnancy; delay owing to disproportion; weight of child, 10lbs. 8oz.; circumference of fœtal head, $16\frac{1}{4}$ inches; a fine healthy child. This woman was delivered in the hospital a year and a half previously, by the natural efforts, of a boy; weight 7lbs. 9oz., after a labour of 12 hours' duration.

CRANIOTOMY.

Craniotomy had to be performed in two instances. The first was aged twenty-three, her third pregnancy. There was distinct projection of the promontory of the sacrum; her two previous deliveries had to be effected by perforator and crotchet. In this labour the membranes had ruptured early in the first stage previous to admission; found it impossible to turn, the uterus being so firmly contracted. Discharged the ninth day. The second, aged twenty-seven, her fourth pregnancy, had malignant disease of the uterus; admitted with intensely fœtid discharge; the fœtal head was distended to an enormous size by fluid from extreme putridity. Died in 29 hours after delivery. *Post-mortem* examination—the uterus was found in a state of sphacelus over its whole extent of surface.

EVISCERATION.

Evisceration had to be resorted to in one case, aged twenty-seven, third pregnancy; admitted with accidental hæmorrhage in

the seventh month; the hæmorrhage had commenced three days previously; complaining of great supra-pubic pain; vaginal discharge extremely fœtid; shoulder found presenting; attempted to introduce the hand, in order to turn, but found it impossible owing to the contracted state of the uterus; so was obliged to eviscerate, and even then there was great difficulty in getting down the body, and in order to deliver the head had to apply the forceps; made a good convalescence.

VERSION.

There were nineteen cases in which we were obliged to perform version, viz.:—6 cases of presentation of the upper extremity; 2 cases of pelvic deformity; 1 case of ruptured uterus; 4 cases of placenta prævia; 3 cases of prolapse of funis; 1 case of accidental hæmorrhage with prolapse of funis; 2 cases, each being the second of twins.

Thirteen children, viz., 5 boys and 8 girls, were born alive; 6 children, viz., 2 boys and 4 girls, were dead born, one being putrid.

Two mothers died—one the case of ruptured uterus, which had occurred previous to admission; the second was the case of placenta prævia; admitted in a state of great exhaustion. Died in 2 hours.

PROLAPSE OF FUNIS.

There were 12 instances of prolapse of the funis; in 5 version was performed, 4 girls being alive, 1 dead; it was in the case of accidental hæmorrhage; in 4 the labour was quick, and the children were expelled by the natural efforts, 2 boys alive and 2 boys dead; in 1 the prolapse took place when the os was the size of a shilling; could not be reduced; the second, the funis was prolapsed and pulseless on admission; in 1 (a case of twins) breech presentation; funis pulseless on admission, delivered of a girl, dead; 1 (a footling case) was delivered of a girl, living; in 1 the head was at the brim, delivered with the long forceps, of a boy, living.

Thus, in the 12 cases, there were 5 boys, 3 of which were saved, and 7 girls, 5 of which were delivered alive; and all the mothers recovered.

PLACENTA PRÆVIA.

There were seven cases of unavoidable hæmorrhage.

All were admitted in a state of great exhaustion; in one case the hæmorrhage commenced two months, and in another three weeks, before admission.

There were three deaths, two died within two hours, and one in eight hours, after delivery. One of the cases that recovered was a case of triplets. She was admitted in the lowest possible state of weakness, as much from hæmorrhage, which had been going on for three weeks, as from want of proper sustenance; in fact, on being carried into the ward we had no idea but that she would have succumbed at once. Her history is as follows:—She was aged forty; her ninth pregnancy; husband a labourer; being out of employment; has been in a state of penury for last three months. She was ordered at once an enema of beef-tea and brandy, and as much as we could get her to swallow by the mouth, which was most difficult. On examination the os was found about the size of a shilling; the placenta could be felt immediately within it. She was plugged, and the strength supported. As soon as the os was sufficiently dilated to admit of it, the hand was passed at the side of the placenta. Membranes ruptured, a leg seized, and child delivered. It was a girl, putrid, weighing 4 lbs.; a second was now discovered, presenting a foot which was delivered; it was a boy, also putrid, weight 4 lbs.; a third was then expelled by the uterine efforts; it was a girl, dead, weight, $3\frac{3}{4}$ lbs. There were three placentæ, all of which were of the battle-door variety. The first was expelled in twenty minutes after the birth of the second child; the other two in a quarter of an hour after the birth of the third child. The mother made a slow recovery, as might be expected, but eventually went out well.

ACCIDENTAL HÆMORRHAGE.

We had 11 instances of accidental hæmorrhage; all occurred in multipara, viz., 2 in 2nd pregnancy; 1 in 3rd, 2 in 5th, 4 in 6th, 1 in 10th, and 1 in her 11th pregnancy. Eight of the children, three male, and five female, were dead born; ten of the mothers recovered. The one fatal case was admitted in a state of great exhaustion, and died from extreme debility.

CONVULSIONS.

Convulsions occurred in five instances—2 in primipara, 3 in multipara—viz., 1 in her 2nd pregnancy; 1 in her 4th, and 1 in the 5th pregnancy. One died. A primipara admitted comatose, having had some twenty fits before admission, and recurring every twenty or thirty minutes; she was delivered with the forceps, but the fits continued until death, having had fifty-three in all. She was treated in the general way, by cold affusion, external stimulants,

such as sinapism to nape of neck, calves of legs, &c. Turpentine and assafœtida enemata, bromid of potass., with ext. belladonna, by mouth or enema, according to circumstances; strength supported by beef-tea, &c. She died in thirteen hours after delivery.

PERINÆUM INCISED.

In two instances we were obliged to incise the perinæum, in consequence of its great elongation and toughness preventing the expulsion of the fœtus. The first, aged twenty-nine, her second pregnancy; the head descended on the perinæum, distending it to such an extent that it measured 7 inches from the verge of the anus to the fourchette, and rendering it so thin that the nose of the child protruded through it, leaving a portion, anteriorly, fully $2\frac{1}{2}$ inches in length. This was cut with a pair of scissors; when the child was at once expelled; it was a girl, $8\frac{1}{2}$ lbs. There was no laceration beyond the incision; the mother made a good recovery. The second, aged thirty-five, her fourth pregnancy—the perinæum in this instance was extremely unyielding, in consequence of a cicatrization, the result of an operation for laceration, having been performed two years previously. The head descended to the outlet, but the perinæum being extremely rigid, we were obliged to incise it to the extent of 2 inches, when the child, a boy, 7 lbs. 15 ozs., was at once expelled. No laceration whatever took place beyond the incision, and her convalescence was most favourable.

In conclusion, having now laid before you the foregoing brief report of the great institution over which I have the honour to preside, I have only to add that, in my opinion, it is by plain, unvarnished, well authenticated statements, such as these, embodying the actual results of practical observation, and extending over a large series of cases, that the progress of obstetric science can be best served. I have laid before you our practice (in a summary manner, no doubt) in almost every variety and complication of labour, and having pointed out the results, I think we need not shrink from criticism on it, nor fear that the Dublin Hospital School of Midwifery will be found behind in the advancement of the obstetric science, which it has always been our aim to foster and improve.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

RECENT WORKS ON CHOLERA.

1. *A Treatise on Asiatic Cholera.* By C. Macnamara, Surgeon to the Calcutta Ophthalmic Hospital. London: John Churchill and Sons. 1870. 8vo, pp. 557.

2. *A Report on the Microscopic Objects found in Cholera Evacuations, &c.* By Timothy Richards Lewis, M.B., Assistant Surgeon Her Majesty's British Forces, attached to the Sanitary Commissioner with the Government of India. Calcutta: Office of Superintendent of Government Printing. 1870.

3. *Report on the Sanitary Administration of the Punjab*, 1868. Lawrence Press. Lahore: 1869.

4. *Report on the Sanitary Administration of the Punjab*, 1869. Lahore: Lawrence Press. 1870.

5. *Cholera in the East, from the commencement of European connexion with it.* By John Macpherson, M.D., Inspector-General of Hospitals, H.M. Bengal Army (Retired List). London: T. Richards. 1869.

6. *Statistics of Cholera.* By Edward Balfour, Deputy Inspector-General of Hospitals, Madras Army. Madras: M. A. and P. Company. 1870. 8vo, pp. 89.

7. *Acholic Diseases, comprising Jaundice, Diarrhœa, Dysentery, and Cholera.* By Alexander Charles Macleod, Surgeon-Major on H.M. Madras Establishment. London: John Churchill and Sons. 1866.

8. *Cholera Maligna is a Specific Acute Inflammation of the Mucous Tissue of the Small Intestines.* By George Barnard, Staff Surgeon, H.M. Indian Army. Calcutta: Thacker, Spink, and Co. 1869. Sewed.

9. *Le Choléra, Etiologie et Prophylaxie, origine, endémicité, transmissibilité propogation, mesures d'hygiène, mesures de quarantaine et mesures spéciales a prendre en Orient pour prévenir de nouvelles invasions du choléra en Europe, Exposé des travaux de la Conférence Sanitaire Internationale de Constantinople.* Par A. Fauvel, Délégué du Gouvernement Français à la Conference, Médecin Ordinaire de l'Empereur, &c. Paris: J. B. Baillière et Fils. 1868. 8vo, pp. 673.

10. *Des Grandes Épidémies et de leur Prophylaxie Internationale avec le Texte des Lois, Décrets, Arrêtés, ordonnances et Instructions qui s'y rattachent.* Par Leon Depautaine, Docteur en Médecine. Paris: J. B. Baillière et Fils. 1868.

11. *Cholera: its Seat, Nature, and Treatment.* By Charles Shrimpton, M.D., Chevalier de la Légion d'Honneur. London: John Churchill and Sons. 1866.

12. *Circular No.* 1. *War Department, Surgeon-General's Office, Washington. Report on Epidemic Cholera and Yellow Fever in the U. S. Army during* 1867.

13. *A Treatise on the Origin, Nature, Prevention, and Treatment of Asiatic Cholera.* By John C. Peters, M.D. New York: D. Van Nostrand. 1866. Fcap. 8vo, pp. 162.

14. *Transactions of the Epidemiological Society of London.* Vol. III. Part I. London: Robert Hardwicke.

15. *On Cholera and Choleraic Diarrhœa.* Two Lectures. By George Johnson, M.D., London, Professor of Medicine in King's College, &c., &c. London: John Churchill and Sons. Sewed.

16. *On Malignant Cholera.* By Edwards Crisp, M.D., &c. London: Robert Hardwicke. 1866.

In the summer of 1866 cholera was a subject of great and immediate interest to every British physician, and we devoted to it a long Review in the journal for August of that year; since then an elaborate report of the epidemic of 1865-66, written by Dr. J. Netten Radcliffe, appeared in the appendix to the Eighth Report of the Medical Officer of the Privy Council, an account of the disease as it appeared in this city, was given in a series of papers read before the Medical Society of the College of Physicians, and subsequently

published in this journal in May, 1867, much valuable information bearing on the subject has been communicated to British and foreign periodicals, and numerous works on cholera have been published; in the questions involved we ourselves take special interest, not alone on account of their direct bearing on the individual disease, but also because they touch more or less nearly other endemic and epidemic diseases; happily, however, at present few of our readers have cause to concern themselves much about cholera, and we do not feel justified therefore in doing more than directing attention to the works which have lately reached us, that those to whom the subject is of interest may know where to seek information regarding it.

The first eight books on our list are written by medical officers of the British army, who are, or have been resident in India; the ninth, tenth, and eleventh by physicians who have chiefly seen the disease in France; the twelfth and thirteenth come from the United States, and the remaining three are English.

We have read Dr. Macnamara's Treatise with great care, and with much interest; it is the most complete and most practical work on Asiatic cholera which we possess; its author adds to an extensive acquaintance with the literature of the subject a thorough familiarity with the disease acquired by personal observation, aided by all the appliances for research, which modern science has given us, and no physician who is likely to meet the terrible disease of which it treats should be without it.

Dr. Lewis's Report on the Microscopic Objects found in Cholera Evacuations is evidently the work of one thoroughly competent for such a duty as that assigned to him; the illustrations are most creditable to all concerned in their production, and we hope Dr. Lewis will have opportunity and leisure afforded to him to continue such scientific investigations as those of which the present report furnishes an account.

The Reports of Dr. De Renzy on the Sanitary Administration of the Punjab contain original information and suggestions of the utmost value to all concerned in Indian sanitation; he works in a field which has been left almost uncultivated, and we hope to hear in subsequent reports that his suggestions have been acted on, and have produced the good results which he anticipates.

Dr. Fauvel's work contains an account, drawn up in the most complete manner, of the Proceedings of the International Conference which assembled at Constantinople for the investigation of the

Epidemic of 1865-66, and must remain a standard work of reference.

The twelfth on our list is one of a series of valuable reports which we owe to the courtesy of the Surgeon-General of the United States Army.

Looking back to the Review in 1866 which we think fairly represented our knowledge at that time, we have no hesitation in saying that we have gained much since that year; many facts which seemed utterly perplexing have been explained, theories which were unsound, and which served to paralyse efforts at prevention, have been shown to rest on no sure foundation, and the truth of doctrines as to the spread of the disease, which at that time gained but a doubtful assent and led to but feeble measures, has been established; it is true that before attempting to answer many questions of great interest in connexion with the pestilence we must await further observations, for even yet we have little but the dim and often treacherous ray of speculation to guide us, nay more, there is considerable difficulty in reconciling with one another some of the facts which individually have been most surely established; but, on the other hand, problems of the greatest importance to mankind have been solved, and though the separate facts ascertained have not as yet been connected with one another, so as to form a harmonious whole, or to justify us in constructing a complete etiology and pathology of cholera, yet they point most distinctly to the measures which those who meet the disease should adopt. That the cholera-giving material comes from the intestines of those who are ill of the disease, that the great agent in the transmission of cholera from place to place is human intercourse, and that the great vehicle for its diffusion through any particular locality, when once imported, is water contaminated by choleraic dejecta, are such facts, on a firm conviction of these are founded the elaborate suggestions of the Constantinople Conference, and Dr. Macnamara's earnest recommendation of sanitary reforms in India; for collecting such a vast amount of evidence in support of these views the public are deeply indebted to him, but to most of his readers the most interesting facts in his work are those which bear on the natural history, so to say, of the cholera-giving material, he describes the influence of variations in temperature and moisture upon it, he examines the views of Pettenkofer, and by showing that it is only during a certain stage in the decomposition of the cholera stuff that it possesses infecting power, and that there are

certain conditions under which it becomes harmless if swallowed, he affords an explanation of many perplexing facts in relation to the disease. To these interesting researches we cannot, however, under present circumstances, further advert.[a]

In describing the results of the microscopic examination of cholera dejecta, Dr. Macnamara and Dr. Lewis agree in denying the existence of fungi peculiar to cholera stools, as maintained by Professor Hallier, but differ in this, that while Dr. Macnamara states, as did Beale in his communications to the *Medical Times and Gazette* in 1866, that the flocculent masses found in the stools consist of epithelium shed by the intestinal villi, Dr. Lewis maintains "that the flakes and corpuscles in rice-water stools do not consist of epithelium, nor of its *débris*, but that their formation appears to depend upon the effusion of blood-plasma."

Saline injections into the veins are among the measures Dr. Macnamara recommends in the treatment of cholera, and we certainly think they should receive a renewed trial, now that the operation of injection has been rendered so much more simple by the invention of the instrument described by Dr. Robert M'Donnell in the last number of this Journal.

In the next edition of Dr. Macnamara's Treatise, we hope to see a carefully prepared Index added to it.

Dr. Johnston has done much for scientific medicine, and his views will always receive from us careful consideration. It was not, however, without regret we read his pamphlet now before us. It consists of two lectures delivered to young men going to the tropics as missionaries. It contains much useful advice as to the hygienic measures so necessary there. With this we find no fault, quite the reverse, but the greater part of the lectures is occupied with an exposition of his views on the pathology of cholera, and a condemnation of the use of opiates early in choleraic attacks. The former is not nearly so lucid as that contained in his *Notes on Cholera* in which his views are explained with

[a] Among other details Dr. Macnamara mentions the power of stopping the decomposition of choleraic dejecta possessed by sulphate of iron, and refers to the statements of Dr. W. Budd and of a member of the International Conference, to the effect that when used for the disinfection of cesspools and latrines, no fresh cases of cholera were manifested, and in every house where it was used as a prophylactic not a single seizure occurred. In justice to Dr. John Brown, an officer of the Indian medical service, who, we fear, is now dead, but who was, in 1856, officiating residency surgeon at Nepal, we are anxious to state that in that year he administered sulphate of iron to the persons of whom he had medical charge in that remote situation, and published a report, which we have now before us, showing its efficacy as a prophylactic.

singular clearness and force. These views have been extensively criticized, and it has been shown—we do not say that his theory is baseless—but at least that some of the supposed facts on which it is constructed (notably the supposed entry of the cholera poison into the blood) are mere assumptions, and what is much more to the point, we find distinguished physicians, whose daily life is spent in the endemic area of cholera, declaring with singular unanimity that the treatment which on that theory is recommended has been found not only useless but mischievous. Surely, with these facts before him, Dr. Johnston should have hesitated to instil into the minds of non-professional men, with dogmatic assurance, notions which will, in all probability, lead them during after life in India, and at times when divided counsels mean death, to set themselves in opposition to professional advice, and to do so with the obstinacy conferred by first impressions and imperfect knowledge.

Dr. Peters's little work contains an exceedingly well-written *resumé* of the literature of cholera, with a full and excellent account of the preventive measures which should be adopted.

Lectures and Essays on the Science and Practice of Surgery. By Robert M'Donnell, M.D., F.R.S.; Fellow of the Royal College of Surgeons in Ireland; one of the Surgeons to Doctor Steevens' Hospital, Dublin. Part I. Dublin: Fannin and Co. 1871. 8vo, pp. 133.

With sincere pleasure have we received this, the first of a series of Lectures and Essays on Surgery, which Dr. M'Donnell intends to publish. The success with which he has cultivated physiological investigation, the acquaintance which he is known to possess with British and Foreign Surgical Literature, and the ample field of observation which he enjoys, eminently fit him for discussing the great surgical questions of the day. His position now entitles him to speak upon them with authority, and accustomed as he has been from the commencement of his professional career to teach, he has acquired a rare facility of conveying to others the knowledge he has himself acquired, and the opinions at which he has himself arrived.

The volume now before us consists of five Clinical Lectures on Venereal Diseases. In the first two the questions now so eagerly disputed are defined and considered. In the third the constitutional

manifestations of syphilis are described, and the fourth and fifth are occupied with treatment. In them will be found a clear and full exposition of the important subject of which they treat.

Études de Médecine Clinique, faites avec l'aide de methode graphique et des appareils enrigistreurs. Par P. LORAIN. *Le Pouls ses variations et ses formes, diverses dans les Maladies.* Baillière et Fils. Paris. 1870.

Studies in Clinical Medicine, made by the aid of the Graphic Method and Registering Apparatus. By P. LORAIN. *The Pulse: its variations and different forms in Diseases.* Baillière, Paris. 1870.

THE graphic method of illustrating the variations of physiological phenomena and symptoms of disease, has received such an impetus by the addition of many recently invented instruments of precision for use in physical diagnosis, that our works on practical medicine bid fair to have the space formerly devoted to lengthy descriptions of symptoms, and long columns of figures, filled by diagrams, which, at a glance, convey a meaning which the reader would take long to deduce from the most lucid description, or the best arranged statistical tables. To those unaccustomed to the graphic method of recording results, many of the curves used to record clinical observations may appear obscure, but when the simple principle upon which these graphic diagrams are constructed is mastered, they at once become a *truly* graphic method of description.

As a means of comparing contemporaneous trains of symptoms, such as the relations between pulse, respiration, and temperature, as to frequency, quickness, and degree, this method is especially useful, but this use of the graphic method has been sufficiently illustrated in the pages of this journal to convince our readers of its clearness and utility.

To M. Lorain belongs the merit of having attempted to bring together and carefully arrange the clinical phenomena, which can be satisfactorily treated by the graphic method. The work before us is the first instalment of a series of memoirs upon this method. The author promises, in addition to this work upon the pulse, others, on the temperature of the body, the composition of the urine, &c.

The author has very properly selected the pulse as the subject for the first part of his work. We have always been accustomed to consider the pulse as the general indicator of the state of our patient, and the more we examine the variations as written down by the pulse-wave itself by means of the sphygmograph, the more we are convinced of the value to be placed on the variations of primary greatness, softness, and hardness, as indicating the real condition of the circulation.

M. Lorain's book contains a full account of all the known means of taking pulse-turnings, shows the pulse-tracings of health and the variations consistent with health; the alternations produced by diseased heart or arteries, and the effects of acute febrile diseases on the pulse-curve.

All the above mentioned points are treated of in the fullest manner, but as M. Lorain has thoroughly practised what he preaches, and relied chiefly on the graphic method instead of words to convey most of his ideas and conclusions to his readers, it would be impossible to do full justice to the value of his work without reproducing his diagrams in a way that would be not only unfair to his work, but quite too extensive for the pages of this journal.

In future the clinical physician must use the graphic method of illustration at his lectures, and M. Lorain has put him in possession of a valuable work to assist his illustrations, and without which he will find himself wanting in one of the most useful implements for instruction in clinical medicine.

The Satires of Horace. Translated into English Metre, by ANDREW WOOD, M.D., F.R.S.E. Edinburgh: W. P. Nimmo.

"QUOD medicorum est promittunt medici" is a canon the force of which we most heartily acknowledge; but, still, we hope we shall not lay ourselves open to the censure, current in legal circles, of "travelling out of the record," if we—whose special province it is to deal with all that relates to the art of healing—venture to commend to the favourable notice of our medical brethren this very attractive volume, which has recently come into our hands.

In this utilitarian age it is the fashion—too much the fashion—to decry, if not to denounce, the spending of time upon the acquirement of classical learning. Clerics, it is said, may reasonably

devote themselves to the study of Greek and Latin authors, and black-letter lawyers may profitably do the same, but what have Doctors or Surgeons to do with dead languages? We can have all the wisdom, if wisdom there be, in Hippocrates, and Galen, and Celsus, in our own tongue, and why trouble ourselves with difficulties and perplexities of interpretation which we may well avoid?

We confess we are not of this opinion, and therefore it is with pride and pleasure that we welcome a contribution to the literature of the age, creditable alike to himself and to the profession, in which he holds a conspicuous place, from the pen of Doctor Wood.

The versification of the Satires of Horace is rugged, rough and ready it may be, but the vigorous good sense which pervades them all is pleasingly brought before us in Doctor Wood's rendering, which brings us face to face with the lively and acute original, sometimes in a shape as rough, and for that reason all the more acceptable, as being an exact transcript. In some instances we may, perhaps, feel inclined to differ from our translator, but the "paucæ maculæ" need not offend "ubi plura nitent."

On the whole, we feel warranted in assuring our readers that they may promise themselves much enjoyment from the perusal of this latest effort to popularize an author beloved and admired by every scholar. It is to be hoped that many, seeing him in this agreeable shape, may be tempted to know something of him in the original; and that those who have made acquaintance with him in early life, may be led to try back and say "hœc olim meminisse juvabit."

A Handy-Book on Health, and How to Preserve It. By Charles A. Cameron, M.D., Professor of Hygiene in the Royal College of Surgeons in Ireland. Second and Revised Edition. London: Cassell, Petter, and Galpin.

People are at length becoming thoroughly alive to the importance of knowing something about the air they breathe, the water they drink, the food they eat, the clothes they wear, the houses they live in, the exercise they take, and the dangers to which their want of any knowledge of the conditions of health exposes them. They are now really convinced that prevention is better than cure, and that ignorance is not bliss, since it makes

them the victims of many a malady which they might easily avoid. They have, therefore, speedily exhausted the first issue of Dr. Cameron's most excellent *Handy-Book of Health*, and it now appears revised and improved as a second edition.

Note-Book of Materia Medica, Pharmacology, and Therapeutics. By R. E. SCORESBY-JACKSON, M.D., F.R.S.E., &c. Second Edition, by Dr. ANGUS MACDONALD, M.A. Edinburgh: Maclachlan and Stewart. 1871. Pp. 686.

THE favour accorded by general consent to the first edition of Dr. Jackson's work renders it a pleasant task to welcome a second edition, revised and brought up to the present state of knowledge, and all will regret that its accomplished author has been prematurely carried off before he had time to gather in the full reward of his labours. Dr. A. Macdonald has been entrusted with the preparation of the new edition, and while the original plan of the work has been strictly adhered to, there are everywhere signs of careful revision, while many of the articles, *e.g.*, those on bromide of potassium, sulphurous acid, nitrate of bismuth, arsenious acid, conium and digitalis, have been almost entirely re-written. Several additions have been made, including, of course, hydrate of chloral and carbolic acid, and in the explanation of chemical re-actions, the modern notation is exclusively employed.

The Note-book, as it now stands, is an accurate, useful, and complete Student's Text-book, and is in every way deserving of a continuance of that popularity which it has already attained.

Method and Medicine. An Essay, by B. W. FOSTER, M.D., Professor of Medicine, Queen's College, Birmingham. London: Churchill. Pp. 59, cloth.

PROFESSOR FOSTER'S erudite essay was read before the Speculative Society, and by them published in their yearly volume. It was, however, well worthy of republication in a separate form. The opening paragraph contains the argument of the essay :—

"A celebrated physiologist recently began one of his lectures with the assertion that 'la médecine scientifique' did not exist; and, not very long after, a distinguished French surgeon expressed in the pages of the album-number of the *Figaro* his satisfaction at the renunciation of

scientific methods in the study of surgery. Two such remarkable statements naturally excited much attention. They were greedily seized upon by sprightly journalists, whose pens, like the hands of Ishmael of old, waging war against every one, rejoiced to find in the unsatisfactory position of medicine a suitable object for attack. Paris soon knew all these writers could tell of the shortcomings of medicine, and the bruit of the discussion reached even London ears."

These opinions of Bernard and Nélaton are combated with much ability, and the usefulness of physiological knowledge in the diagnosis and treatment of disease is forcibly set forth. With the following words the essay concludes:—

"Now chastened by repeated failures we have abandoned all inquiries into final causes as a search beyond the powers of the human mind, and concentrating our attention on the laws which regulate the succession and relation of phenomena, we are content to move more slowly and more surely towards that perfect wisdom, whence comes perfect action. In this new stage of growth medicine, less and less depending on the blind gropings of empiricism, and no more subject to metaphysical systems, will learn to apply to the great problems of health and disease the invariable laws of science; then the physician, no longer condemned to contemplate in miserable inaction the progress of a disease whose course he cannot control, will defeat by exact knowledge the subtlest approaches of his foe: 'homo minister et interpres naturæ, quantum scit, tantum potest.'"

L'Herpétisme, Pathogénie, Manifestations, Traitement, Pathologie Expérimentale et Comparée. Par le Dr. L. Gigot-Suard, Médecin Consultant aux Eaux de Cauterets, &c. Paris: Baillière. 1870. Pp. 468.

The study of diatheses is carried to a much greater extent by French than by British authors, and the connexion with, and dependence of particular diseases on certain constitutional conditions is strongly insisted upon by some of their foremost writers. We ourselves seem to be tending towards a more emphatic recognition of such a filiation than has hitherto prevailed, and perhaps the best example of this is seen in the branch of dermatology, wherein an attempt is being made to interpret the correlation of many apparently local affections with the constitutional state of the individual.

M. Gigot-Suard has pushed such a line of thought to its

extreme limits, and as the result of his experience at one of the most frequented thermal resorts in Europe, has acquired the conviction that most of the chronic affections which passed under his observation are only varieties of a single type, are but manifestations of a single constitutional disease, viz., herpetism. The subject matter of the book will best appear by quoting the author's definition of herpetism (p. 60). "A chronic constitutional disease, hereditary, or acquired, non-contagious, continuous, or intermittent, characterized by various manifestations which are produced simultaneously or alternately on the skin and other organic systems, which manifestations are directly caused by the presence of excrementitious materials in excess in the blood, especially of those which normally occur in it in very small quantity, and which are not excreted by the skin, *i.e.*, urates, oxalates, hippurates, xanthine, creatine, &c." It should be premised that M. Gigot-Suard disbelieves in the existence of a particular "arthritic" diathesis, and discards entirely the use of that term, so familiar in the writings of Bazin. By far the greater portion of the work is devoted to the description and symptomatology of the special manifestations of herpetism, *i.e.*, the *herpetides* which, according to the author, embrace almost every known type of disease, if we except some few classes, such as the strumous, parasitic, syphilitic, and leprous affections.

The discussion of the cutaneous herpetides tempts M. Gigot-Suard to offer a new classification of dermatoses which he believes to be more simple, more practical, and withal more conformable to observation than any which have hitherto been proposed. But as our readers might not share in this opinion, we will not reproduce it. Like too many French works, the present treatise is painfully diffuse, and needlessly padded with copious quotations, taken almost exclusively from French sources, but, unlike many, it is fortunate in the possession of an index as well as a classified table of contents.

The Medical Works of Francisco Lopez de Villalobos, now first translated, with Commentary and Biography. By GEORGE GASKOIN. London: J. Churchill and Sons. 1870. Pp. 312.

LOPEZ DE VILLALOBOS, poet and syphilographer, was an eminent and learned physician, attached to the Court of Spain, who flourished in the latter part of the fifteenth, and first half of the

sixteenth centuries. He was born in 1473-74, and probably died about the middle of the sixteenth century, but exact information on the details of his life is wanting. Mr. Gaskoin has employed his leisure in translating and annotating some of the "Licentiate's" works, which, from their rarity and early date, possess some interest for the student of the history of medicine. Of Villalobos' principal work, *i.e.*, *El sumario de la medicina con un tratado sobre las pestiferas bubas* (*i.e.*, syphilis), but three copies are known in Spain, and one exists in the British Museum, dating from the Salamanca press, in 1498, twenty-four years after the first work printed in Spain. Besides the main poem, consisting of seventy-four stanzas, two dialogues are appended, one on The Tertian Fever, and the second on Natural Heat, which form part of the "*problemas*," the last publication of Villalobos, which passed through five editions in the sixteenth century.

☞ *We have to announce that Dr. MacCormac has in press a further Treatise on the origin of Consumption, to wit—"Rebreathed Air and Disease."*

PART III.

QUARTERLY REPORTS.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.[a]

By Walter G. Smith, M.D., Dubl.; Fellow and Censor, K. & Q.C.P.I.; Senior Demonstrator of Anatomy in the School of Physic; Assistant Physician to the Adelaide Hospital.

MATERIA MEDICA AND GENERAL THERAPEUTICS.

1. *Incompatibility of Iodide of Potassium and Nitrate of Bismuth.*—Drs. Woodman and Meymott Tidy call attention to the decomposition which takes place when potassic iodide is mixed with subnitrate of bismuth. A dark red iodide of bismuth is gradually formed, nitrate of potassium remaining in solution. The red precipitate is a very insoluble substance, yet is slightly soluble in acetic acid. When a concentrated solution of potassic iodide is added to the bismuthic powder, the red precipitate appears almost at once, but in a more diluted state, and with the nitrate of bismuth diffused through water, a yellowish precipitate is slowly formed.—(*Brit. Med. Journ.*, Dec. 3, 1870.)

2. *Purity of Drugs.*—(α). *Iodide of Potassium.*—From a report of the analyses of six samples of this drug, obtained in various parts of London, it appears, first, that commercial iodide of potassium is almost perfectly pure, and is remarkably free from bromide and chloride; and second, that, as actually sold, it is very dry. The percentage of absolute iodide ranged from 97·7 to 99·3. There are two varieties of iodide of potassium—the *opaque* and the *transparent* crystals. Fashion has led to the opaque kind being preferred, but the transparent crystals are just as pure.—(*Brit. Med. Journ.*, July 23, 1870.)

[a] The author of this Report, anxious that no contribution to the subjects of Materia Medica and Therapeutics should remain unnoticed, will be glad to receive any publications which treat of them. If sent to the correspondents of the Journal (see page iv.) they will be forwarded.

(β). *Bromide of Potassium.*—In the two last reports (Febr. and Aug., 1870) it was shown that bromide of potassium, as sold in England, Scotland, and France, is extensively and largely adulterated with various impurities, sometimes to the extent of 31 per cent. On the other hand, a report of the analyses of six samples of this salt is published in the *Brit. Med. Journ.*, Aug. 13, 1870, from which it appears that the bromide of potassium, now used in medicine, is remarkably pure. Out of the six specimens, only one contained a minute quantity of iodine. An opaque sample was found to include 2·7 per cent. of water, and in the worst of the samples examined there was not more than 5·2 per cent. of chloride of potassium.

(γ). *Saccharated Carbonate of Iron.*—Mr. Nicholson examined 24 specimens of this compound, and found that the proportion of carbonate of iron varies greatly, and descends from 41·9 per cent. to 22·61 per cent. Ferri carb. sacch. should theoretically contain 45·5 per cent.—(*Pharm. Journ.*, Sept. 24, 1870.)

(δ). *Citrate of Iron and Quinine.*—Analyses of 10 specimens of this double salt, obtained from different manufacturers, are furnished in the *Practitioner*, Oct and Dec., 1870. Taking the Pharmacopœia as a standard, the citrate should contain 20 per cent. of peroxide of iron, and 16 per cent. of quinia. All the specimens contain the due amount of ferric oxide, in some cases exceeding it, and in one case, the percentage of ferric oxide is as high as 33. But the proportion of quinia varies greatly, and though in 3 samples, Nos. 1, 2, and 7, the percentage of quinia is even slightly above the prescribed quantity; in others, *e.g.*, Nos. 5 and 6, it falls nearly to 4 per cent.

In the *Pharm. Journ.*, Sept. 1868, Mr. Braithwaite published a report on the quantities of alkaloid in various specimens of citrate of iron and quinia, and arrived at somewhat similar results.

(ε). *Saffron.*—M. Blachez has detected the adulteration of saffron with carbonate of lime or chalk, coloured with some matter, probably vegetable (*Journ. de Pharm. et de Chim.*, Avril, 1869), and Mr. D. Hanbury calls attention to the same adulteration in the *Pharm. Journ.*, Sept. 24, 1870. One specimen contained 28·01 per cent. of calcic carbonate. M. Constantin, of Brest, has also pointed it out in *L'Union Pharm.*, Aug. 1870.

3. *Veratria.*—The discrepancies in the statements of different observers regarding this drug may, perhaps, be in part explicable

by recent inquiries into the nature of this substance. Mr. C. Bullock, Philadelphia, has discovered that the so-called veratria is a complex body, and contains two alkaloids associated with a resin. Professor H. C. Wood has carefully examined the action of these bodies, and names the alkaloids, viridia, and veratroidia; the former of which is soluble in ether, the latter insoluble. Viridia is not an emetic or cathartic; veratroidia is an irritant emetic, and sometimes a cathartic. The resin is inert therapeutically. By means of viridia all the peculiar sedative influence of veratrum viride can be obtained without the risk of producing the nausea and vomiting which occasion so much discomfort and even danger.—(*Amer. Jour. of Med. Sci.*, Jan., 1870.)

Veratrum viride exerts its influence chiefly on the circulation, reducing the pulse most markedly, and M. Oulmont concludes that veratrum is a cardiac poison, analogous to digitalis, from which it is distinguished by its extraordinary rapidity of action. MM. Oulmont and Pégaitaz have observed that the temperature is likewise diminished (*Rev. de Thér.;* Dec. 1868. *Deutsches Arch. f. Klin. Med.*, Oct., 1869), but Dr. Squarry is unable to confirm this.—(*Practitioner*, 1870.)

4. *Aconite Alkaloids.*—Very soon after the introduction of aconitine into medical practice by Turnbull, differences in the quality of differents pecimens of the alkaloid were noticed, some being much stronger than others, while some would not produce the characteristic numbness described by Turnbull and others.

Since 1864, even, two kinds of aconite alkaloid were obtainable in the London market, and it is stated that the topical action of Morson's preparation is at times much more powerful than at other times. Considerable light has recently been thrown on this important point, by the researches of Professor Flückiger, MM. T. and A. Husemann, and Mr. T. B. Groves. It is now established indubitably that there are at least two distinct alkaloids, aconitia, and pseudaconitia. Aconitia is contained in the blue-flowering European and Himalayan species of Aconitum, especially in A. Napellus. Pseudaconitia (Von Schroff) is an entirely different body, and is found especially in the aconite roots (bikh) of Nepaul, derived from the A. ferox (Groves). Pseudaconitia, when applied externally, acts like veratria; aconitia does not. They both depress the action of the heart, but pseudaconitia is much more energetic, and is probably at least 12 times as powerful as aconitia. Pseudaconitia differs

physically and chemically from aconitia. English "aconitia" appears to owe its reputation to the presence or substitutiou of pseudaconitia. The two alkaloids might perhaps be introduced with advantage into medical use; pseudaconitia being the one adapted for the treatment of neuralgia.—(*Pharm. Journ.*, Aug. 16, Nov. 12, Nov. 26, 1870.)

5. *Conium.*—Dr. John Harley, already so favourably known by his admirable work, "The Old Vegetable Neurotics," a model of accurate pharmaceutical and therapeutical inquiry, has made some further investigations into the characters and relative medicinal value of the preparations of Hemlock. It is premised that *the only reliable test of the amount of active principle in a preparation of hemlock is physiological action*, since chemical methods, applied to the quantitative estimation of an alkaloid, so subtle and unstable as conia, give only the most fallacious results.

In testing the qualities of this drug, Dr. Harley calls particular attention to the marked variation in effects of the same dose of a given preparation of hemlock upon the same patient, and finds that this is mainly, if not altogether due to two causes, viz., the degree of motor activity of the body, and the rapidity of absorption from the stomach, being inversely as the former and directly as the latter. Eliminating these disturbing influences from his experiments, he is led to conclude:—(1.) That the advantage of the green fruit over every other part of the plant is so clear and decided, that nothing need be said in favour of its selection as the basis of the tincture and extract. The extract of the Pharmacopœia is a scandal to the present state of medical knowledge, and a spirituous extract of the green fruit ought, as soon as possible, to take its place. (2.) The variability of the strength of the succus and its expense must prove serious drawbacks to its use, but these doubtless would be in a great measure removed, if hemlock were regularly cultivated for medicinal use in fixed localities.—(*Practitioner*, Dec., 1870.)

6. *Toxic Action of Quinia.*—In the Reports for February and August, 1870, 5 cases were instanced, in which an erythematous rash, succeeded by desquamation, followed the administration of *small* doses of quinia. My friend, Dr. W. H. Steele has favoured me with the notes of another case. While on duty with his regiment at Hyderabad, January 1870, he was consulted by Mrs. B., who was suffering from quotidian ague, attended with considerable gastric irritability. The lady at once said that she could not take

quinia, for it had been given to her before, and always with the effect of bringing out patches of a red irritable eruption on the skin. No quinia was given for two or three days, but, on seeing that the fever still continued, Dr. Steele prescribed a mixture containing 2 grs. of quinia to the ounce. Shortly after taking the second dose he saw her, and she at once pointed out to him several bright erythematous patches, one on the right hand, another on the knee, and one on the ankle; and from the intense irritation about the nose, she felt sure that a patch would shortly appear there. The quinia was persevered in, and with a happy effect on the ague, but, so long as she continued it, the patches remained. On leaving it off they began to fade, and finally disappeared, the cuticle afterwards desquamating. It is observable that all the cases as yet recorded have occurred in women, usually of middle age, and sometimes, at least, of a nervous irritable temperament. In one instance the eruption appeared within half an hour after taking one dose of citrate of iron and quinia.—(= $1\frac{1}{2}$ grs. citrate of quinia.)

7. *Chloroform.*—(α). *Physiological action of.*—Professor Paul Bert shows (*Leçons sur la Phys. comp. de la Respir.*, 1870) that during the *uncomplicated action* of chloroform the blood is richer in oxygen than during the normal condition; the arterial blood is bright red, and even the venous blood may be of a marked rose-colour. It is therefore erroneous to regard anæsthesia as a state of asphyxia. In the early period of excitement the blood, arterial and venous, is darkened in colour, and if the quantity of chloroform be pushed too far, asphyxia ensues, and the arterial blood assumes a black colour.—(*Journ. of Anat. and Phys.*, May, 1870.)

(β). *Death from.*—Dr. B. W. Richardson points out four modes of death, (1.) Syncopal apnœa; (2.) Epileptiform syncope; (3.) Paralysis of the heart; (4.) Depression from chloroform + surgical shock. The effects of chloroform are exerted successively on the brain, spinal cord, sympathetic centres, and cardiac centres, and the immediate cause of death from chloroform in every case is, in all probability, excitation either of the motor or of the controlling nervous mechanism of the heart. The best mode of resuscitation is steady artificial respiration by the double-acting bellows.—(*Med. Times and Gazette*, July 23, 1870.)

8. *Hypodermic Injection.*—In a paper read before the Clinical Society, Dr. Handfield Jones raised a query as to the *safety* of subcutaneous injections. Three cases were described, in which the

injection of small doses of morphia or opium ($\frac{1}{5}$ gr. of morphia in the first case, 5 m. of liquor opii. in the second, and $\frac{5}{24}$ gr. of morphia in the third) had been followed by more or less serious fainting. He thought it very desirable to ascertain whether morbid changes in the valves, or muscular tissue of the heart increased the liability to the occurrence of syncope, or whether this was chiefly dependent, as chloroform-syncope seems to be, on some latent infirmity of the cardiac nervous centres (*Brit. Med. Journ.*, Dec. 17, 1870). Dr. Clifford Allbutt, from the experience of the last three years, has gradually been led to a consideration of the *abuses* to which the hypodermic injection of morphia is occasionally liable, and invites the attention of the profession to this view of the question. Many patients, who suffer from various forms of intractable neuralgia, glide into the habit of constantly resorting to the use of the syringe for the alleviation of their sufferings, and declare that without it life would be insupportable. Relief follows each time, and the patient is restored to ease and cheerfulness, yet, when the morphia is discontinued, they too often remain as far from cure as ever, and in addition, are distressed by an indescribable depression and irritability which morphia can alone relieve, and for which it is accordingly again and again administered. It then becomes a serious question, whether the injection of morphia, though free from the ordinary evils of opium-eating, may, nevertheless, create the same artificial want and gain credit for assuaging a restlessness and depression, of which it is itself the cause. Dr. Allbutt is chiefly anxious to elicit the judgment of the profession on this matter, and concludes his suggestive paper by a quotation from Niemeyer's *Lehrbuch*, showing that the German physician was aware of the danger of heedlessly continuing the use of morphia-injections.—(*Practitioner*, Dec., 1870.)

Dr. Paul Sale, of Missouri, injected into the arm of a young woman an ethereal solution of quinia, for the relief of an intermittent fever, running into malarial coma. The injection was followed by redness and swelling in the punctured spot; 12 days afterwards tetanus set in, and she died the following day.—(*Med. and Surg. Rep.*, Philad., May 14, 1860.)

9. *Antiseptics.*—(α). *Sulphocarbolates.*—Dr. Sansom again calls attention to the medicinal qualities of the salts of this compound acid, which he considers entitle them to an honourable place in our materia medica. He has obtained sulphocarbolates of the alkalies,

alkaline earths, and of iron, copper, and zinc, and mentions a number of cases in which he has employed one or other of them with apparent benefit. The observations as yet brought forward are too desultory and incomplete to lead to any accurate conclusions; yet, these compounds are deserving of a further trial, since their administration is exempt from some disadvantages which attend the use of carbolic acid itself and its simple salts.—(*Brit. Med. Journ.*, Dec. 24, 1870.)

From some experiments lately devised to ascertain the comparative energy of various substances ordinarily used as antiseptics, Dr. Crace Calvert considers that it is clearly shown that the only true antiseptics are carbolic and cresylic acids; and these results coincide with those obtained by Mr. W. Crookes, Dr. Angus Smith, and Dr. Sansom.—(*Brit. Med. Journ.*, Dec. 24, 1870.)

(β). *Chloralum.*—This substance, the hydrated chloride of aluminum, though not new to chemists, has recently been introduced as an antiseptic and disinfectant. Mr. J. Gamgee claims for it a superiority over carbolic acid, on account of its inoffensiveness and harmlessness, and because the strength can be moderated at pleasure. Dr. Stevenson reports that, in its antiseptic properties, it seems to resemble many other mineral salts, and appears to be inferior to carbolic acid. The name is most unfortunately chosen, and we may expect soon to hear of strange confusion between it and chloral.—(*Brit. Med. Journ.*, Dec. 24, 1870, p. 691.)

(γ). *Sulphites.*—In 1857, Professor Polli, of Milan, initiated the use of the sulphites and hyposulphites as prophylactic and curative agents in zymotic affections, and he has ever since advocated them with untiring constancy. The Royal Institute of Lombardy has proposed, as the subject for a prize essay in 1872, the study of the value of sulphitic medication in the treatment of malarial fevers, as compared with other remedial means already known.—(G. Polli, *Della Malaria e dei Febbrifughe*, 1870.)

10. *Carbolic Acid* (α), *Effects on Urine.*—Some further observations have been published on this curious point in addition to those recorded in the last report (Art. 6-19.) Dr. J. A. Waldenström in all his experience has met with three cases only in which the urine exhibited any peculiar change; two of the patients were affected with gangrenous phlegmon of the leg and the third with a suppurating periostitis in the right thigh. In the first two cases the urine assumed a dark red colour, but no evidence of blood

was obtained. In the third case the darkened urine was found to contain both albumen and the colouring matter of blood. A similar transitory presence of albumen in the urine was observed in a patient who used carbolic acid *internally* for a syphilitic cutaneous eruption, and Neumann has shown that hyperæmia of the kidneys with separation of the renal epithelium constantly occurs in dogs poisoned with carbolic acid (*Med. Press and Circ.*, Oct. 5, 1870). Mr. Aug. Almen has likewise several times observed the tar-like appearance of the urine in patients submitted to the use of carbolic acid. The urine was acid in reaction, turbid, and contained a little albumen but no blood. The colouration probably arises from oxidation of the acid prior to absorption, and, fearing the possibility of nephritis being caused, Mr. Almen strongly recommends the use of very dilute solutions.—(*Med. Press and Circ.*, Oct. 19, 1870.)

(β). *Poisoning by.*—In three cases lately reported in the journals, death has followed the absorption of carbolic acid, and a fourth is noted in which most serious symptoms were developed. A baby, one year of age, was incautiously set down on a block of wood on which a disinfecting solution of carbolic acid (1 oz. acid to 1 quart of water) had been sprinkled. Extensive ulceration of the gluteal and vulvar regions set in next day, sloughs formed, and the child sank on the tenth day (*Brit. Med. Journ.*, Oct. 8, 1870). A young man, aged nineteen, suffering from enteric fever, was accidentally given a teaspoonful of strong carbolic acid (kept for disinfecting purposes). The patient became completely prostrated, and died within two days, having never rallied from the state of depression. During life the skin was livid and mottled, the stools blackish-brown, and the urine dark brown (*Med. Times and Gazette*, Oct. 22, 1870). A young woman, aged twenty-two, suffering from relapsing fever, took by mistake, instead of a cough mixture, half an ounce of carbolic acid (Calvert's No. 4). When visited five minutes afterwards, she was comatose, pupils insensible to light, pulse very feeble, surface of body livid and covered with clammy perspiration. Stools *offensive;* urine greenish-black, non-albuminous. In a few hours she revived sufficiently to be able to swallow, but the cough and disturbance of the breathing, which were evident from the first, continued, and in two days double basic pneumonia set in. The pulse maintained a persistently high level, profuse perspirations came on, with abundant muco-purulent expectoration, latterly very fœtid, and on October 13th, nearly a month after the accident, she

suddenly became worse and died. At the *post-mortem* the lower lobe of each lung was found to be densely hepatized, and each presented a gangrenous excavation. All the other organs were healthy. (*Glasgow Med. Journ.*, November, 1870.)

In the *Canada Med. Journ.*, July, 1870, Dr. Fraser relates a case in which alarming symptoms, similar to those already described, were developed after the application of an ointment (containing one part of carbolic acid to four of lard) to the limbs of an old man labouring under acute eczema. The patient recovered under the use of a stimulating line of treatment.—(*Journ. of Cut. Med.*)

11. *Bromal.*—This substance, which is exactly analogous to chloral in composition, has been investigated by Dr. E. Steinauer. (*Virchow's Archiv.*, July, 1870.) It was discovered by Löwig, in 1832. Dr. Steinauer believes that bromoform is slowly liberated in the blood, and is finally eliminated in the urine, as bromide of potassium or sodium. When doses varying from two to fifteen grains were subcutaneously injected into frogs, rabbits, and guinea-pigs, the symptoms produced were: first, a stage of restlessness, followed by imperfect sleep and anæsthesia, and finally, dyspnœa and death, either with or without convulsions. After large doses the heart was found relaxed and distended, while after smaller doses it was tetanically contracted. The author has tried bromal, in the human subject, in a few cases of epilepsy and tabes dorsalis, in doses of half grain to one and a half grains. The previous administration of ten or fifteen grains of bicarbonate of soda prevents the development of the stage of restlessness by favouring the evolution of the bromoform. (*Med. Press and Circ.*, Sept. 24, 1870.) Dr. Dougall contributes an independent paper to the *Glasgow Med. Journ.*, for November, 1870. Bromal hydrate crystallizes in white transparent crystals, aggregated into minute, stellate groups. Its odour strongly resembles that of chloral, and its flavour is persistently pungent. Soluble in six parts of water, also soluble in rectified spirit and in ether. Decomposed by alkalies in the same way as chloral. The experiments were chiefly made on rabbits and frogs. Its acrid action is shown by irritation of the air-passages, increased flow of the secretions of the eye, nose, and throat; dyspnœa, vomiting, hiccup, restlessness, and convulsions. In the human subject one grain caused unpleasant results, and four grains, given in a case of disease of the aortic valves, produced great depression and severe pain in the stomach. Dr. Dougall is not inclined to think well of

the practical utility of bromal, either as a hypnotic or as an anæsthetic. Dr. T. Fraser has invariably found that the smallest dose which could produce any impairment of sensibility in rabbits, likewise produced great dyspnœa, and ultimately death, while the *post-mortem* revealed an extreme degree of congestion of the lungs and respiratory passages. Some of these effects may possibly be due to impurities in this preparation.—(*Journ. of Anat. and Phys.*, November, 1870.)

12. *Alcohol—Action on Human Body.*—Drs. E. A. Parkes and Count C. Wollowicz have conjointly made two important series of experiments with the object of ascertaining the physiological and dietetic effects of alcohol on the human body in a state of health. The *first* series does not include wine or beer, but refers exclusively to alcohol mixed with pure water, or to brandy. The subject of the observations was a soldier, aged twenty-eight, of remarkably good physical condition and superior intelligence. Some of the more interesting of their conclusions are—(1.) That some point near two fluid ounces of absolute alcohol the limit of useful action on the appetite. (Dr. Anstie had fixed it approximately at $1\frac{1}{2}$ ounces.) Beyond that, on an average, symptoms of narcosis were produced. (2.) With small and with large doses the heart's action is quickened. (3.) It did not seem to impede primary digestion, but four fluid ounces considerably lessened the appetite, and larger quantities almost entirely destroyed it. (4.) Neither pure alcohol nor brandy *lessened the temperature.* The discrepancy of this statement with many previous observations may be in part explained by the moderate quantity of alcohol taken, and by the fact that the individual experimented upon possessed a healthily-resisting and not a diseased or weakened organism. (5.) The *nitrogen excretion does not appear to be affected* (the ingress of nitrogen being constant), nor does the amount of free acidity, of the phosphoric acid, or of the chlorine in the urine seem to diminish, either with normal or with moderately narcotic doses. (6.) A certain amount of elimination of alcohol, unchanged, does go on, but not very large in amount, at any rate, so far as regards the urine; but the experiments do not show whether *all* the alcohol is eliminated, or whether some is destroyed in the body. (7.) *Cæteris paribus*, the weight of the body is not appreciably affected.—(*Lancet*, Aug. 13, 20, 1870; from the *Proc. of Roy. Soc.*)

The *second* series of experiments relates to the action of claret (Haut Brion of good quality), and the general results therefrom are almost identical with the experiments on alcohol and brandy. In other words, claret in quantities of ten to twenty ounces daily, as they gave it, cannot so far be distinguished in its effect from pure alcohol, although it would be going too far to say that the *dietetic* effects of red Bordeaux wine and of dilute alcohol are identical. Their precise relative influence on primary digestion and assimilation would require special investigation by methods not touched upon in the present paper.

These experiments also tend to fix the quantity of alcohol which can be safely taken in health, at a somewhat lower limit. The ten ounces of wine (containing about one fluid ounce of pure alcohol) did not cause any unpleasant symptoms; but twenty ounces (containing almost two fluid ounces of alcohol) were manifestly too much, and caused flushing of the face, drowsiness, and a feeling of discomfort. With regard to the propriety of this individual healthy man taking any alcohol, the authors have no hesitation in saying that he would be better without it.—(*Brit. Med. Journ.*, Dec. 3, 1870.)

The final result of these experiments goes to confirm the opinion held by most physicians as to the indications for the use of alcohol. The effects on the appetite and circulation are the practical points to seize, and the commencement of narcotism probably marks the point when both appetite and circulation will be damaged. In the face of the chemical evidence, it appears unlikely that alcohol can enable the body to perform more work on less food, though by quickening a failing heart it may enable work to be done which otherwise could not be performed. It may then act like the spur in the hide of the horse, eliciting force, though not supplying it. The experimenters, whilst recognizing further the great practical use of alcohol in raising a failing appetite, exciting a feeble heart, and accelerating a languid capillary circulation, are strongly impressed with the need of moderation and caution in its use.—(Abstract in *Pharm. Journ.*, Aug. 13, 1870.)

In connexion with the question of the utility of alcohol in a state of health, it is of interest to note that in the German army each soldier receives daily one bottle of light wine. This includes about two ounces of absolute alcohol, and there can be little doubt that this liberal allowance of alcohol has been valuable in the peculiarly trying circumstances of the campaign (*Lancet*, Nov. 5,

1870.) An elaborate report of the Committee on the Relations of Alcohol to Medicine, by Dr. J. Bell, chairman, has also come to hand. Although impressed with the sanction of the American Medical Association, and covering 120 pages, it is written with such a strong bias towards total abstinence, while no demarcation is drawn between the moderate use and abuse of alcohol, and no additional light is cast on the problems discussed, that it is unnecessary to refer to it more particularly.

13. *Compression of the Vagus Nerve.*—The late Dr. Augustus Waller has published two interesting and valuable papers on this subject in the *Practitioner* for 1870. The first is on the effects of compression of the pneumo-gastric in the cure or relief of various nervous affections; and the second on the application of this method to the production of asthenia or anæsthesia in surgical operations.

The practice of applying what is termed compression of the carotids for the induction of sleep and the relief of hysterical and epileptic attacks was introduced fifty years ago by Dr. Parry, of Bath, and the results obtained by him and others have invariably been attributed to the decreased circulation in the brain consequent on the compression of the vessels. But since the year 1861, Dr. Waller has been led to the conviction that the effects observed in the above case were *owing to the irritation of the vagus and sympathetic nerves*, and in support of this view he urges a convincing mass of physiological and pathological evidence. After a brief recapitulation of the functions of the vagus and cervical sympathetic nerves, he proceeds to show to what extent the symptoms produced by pressure on the neck of man correspond to those observed in animals, and to the anatomical distribution of the vagus nerve. The nerve may be most conveniently compressed at the side of the neck, at or near the angle of the jaw. The thumb, guided by the pulsations of the carotid artery, serves to compress the nerve, the other fingers resting in a row on the posterior part of the neck. It might be doubted whether compression could be so readily directed as to affect the functions of the nerve, but it is easy for any one to assure himself of the truth of this assertion. The first symptom of compression of the nerve is most usually a sensation of want of air, followed by a deep and laborious respiration, the characteristic sign of the innervation of the pneumo-gastric being disturbed. At the same time the heart's action

becomes weakened and its rhythm irregular, and a continuance of the pressure soon retards the pulse by five or six beats in a minute. These phenomena are frequently accompanied by a sense of uneasiness and præcordial distress, and sometimes nausea, or even vomiting, is induced. The symptoms observed in his own person, and in four other individuals, show that by means of pressure it is possible to reproduce on man most of the principal symptoms observed on animals when the vagus nerve is isolated and subjected to the direct influence of galvanism or other irritant. As a *remedial agent*, Dr. Waller has successfully employed vagal compression in some obstinate cases of severe spasmodic vomiting, in nervous disturbance of the heart's action occurring during acute rheumatism, and in protracted trigeminal neuralgia. As a minor application, Dr. Waller confirms Parry's estimate of its value in bringing about sleep in cases of restlessness, particularly when the mind is tormented by one persistent idea of an annoying nature. Physiologically, the study of the various sensations which attend compression of this nerve cannot fail to be of interest, and the physician is now enabled to ascertain the condition of the nerve and its principal branches in obscure affections of the nervous system which has hitherto been a desideratum in practice. In his second paper, Dr. Waller speaks of the results obtained by this method as applied to surgery, premising that the symptoms may be classed under two heads—viz., the asthenic and the anæsthetic. The asthenic symptoms, which sometimes amount to loss of all voluntary power, the patient falling into syncope, present various intermediate degrees of intensity of muscular debility, and a certain amount of debility is almost invariably the result of vagal pressure. In illustration of the value of this plan, Dr. Waller quotes an instance of difficult reduction of a sub-clavicular dislocation of the humerus in a powerful and athletic man. No chloroform was at hand at the moment, and unavailing attempts at reduction had already been made, but with the aid of vagal compression reduction was speedily accomplished. Dr. Waller asserts that compression of the vagus is perfectly free from any danger whatever to life, though such apparently alarming symptoms occur, and so it contrasts favourably with the ordinary anæsthetics, particularly chloroform. As an example of the anæsthetic powers of this method, a case is cited of extraction of a molar tooth; no pain was suffered by the patient. It is a matter of very great regret that the sudden death of Dr. Waller has deprived practical medicine of a most

valuable series of researches on the application of physiology to therapeutics which he had undertaken to carry out, and which he was so competent to fulfil.

SPECIAL THERAPEUTICS.

14. *Belladonna* (α), *Uses and Effects of.*—Dr. W.T. Plant, of New York, in a paper on the history and uses of belladonna (*Medical Record*, April, 1870), records his belief that belladonna and opium are mutually antagonistic and antidotal. He confirms the opinions of Harley and others, who have pointed out that children will bear and often require proportionally larger doses of belladonna than adults, and states that, in its influence over the mammary secretion, it stands almost alone, and is unrivalled. Besides its uses in ocular diseases, he recognizes its utility in hooping-cough, inflammatory affections of the cord, habitual constipation, spasmodic contractions, and in neuralgias, epilepsy, and tetanus.—(*N. Y. Med. Journ.*, Nov.)

(β). *Incontinence of Urine.*—Dr. Burney Yeo records two cases in which the employment of belladonna was completely successful. No. 1 was a lad, aged sixteen, who had suffered from this infirmity since infancy. At first 5 m. tinct. bellad. with 10 m. tinct. ferri perchlor. were ordered three times a day. He improved slightly; but on the dose of tinct. bellad. being increased to 10 minims, immediate good effects followed; and when 15 m. were given, without any iron, the nocturnal incontinence occurred only once in three weeks; 20 m. were finally ordered, rendering vision indistinct, and the incontinence was completely checked, and did not return. There was no irritability of the bladder. No. 2 was a girl, aged sixteen, who had similarly suffered from infancy. The belladonna was administered as before, but without any iron, and after the first week of treatment the incontinence rapidly abated, and now scarcely ever occurs. Dr. Clifford Allbutt confirms the utility of belladonna in similar cases (*Lancet*, Oct. 22, 1870). Dr. J. Barclay specially recommends the *syr. ferri iod.* in this affection, and quotes eleven cases, all of which cured. Dr. Thomson recommends and has been successful with hydrate of chloral.—(*Med. Times and Gaz.*, Dec. 17, 1870.)

(γ). *Spermatorrhœa.*—Mr. R. M. Jones was led by analogy to try belladonna in cases of nocturnal emissions and of so-called spermatorrhœa, consequent upon a depressed and irritable state of the generative organs, and has met with very beneficial results, even in extreme cases.—(*Practitioner*, Jan., 1871, from *Lancet*, Nov. 26.)

15. *Carbolic Acid in Pyæmia.*—In cases of pyæmia and erysipelatous poisoning, Dr. J. Wood advocates the value of keeping the atmosphere round the patient constantly saturated with carbolic acid vapour. For this purpose muslin bags filled with Macdougall's disinfecting powder are placed beneath the bed-clothes, suspended from a cradle, and, if it be thought necessary, the powder is freely sprinkled under and around the bed, and the floors are washed with a solution of the common carbolic acid. In one case the characteristic effects of absorption of the acid were noticed in the breath and urine.—(*Practitioner*, Jan., 1871.)

16. *Digitalis in Delirium Tremens.*—Dr. Wiltshire reports five cases successfully treated by half-ounce doses of tinct. digitalis; four of his patients took one dose only, the other patient first got half-an-ounce, and then, after some hours, two drachms in addition. The effects produced on the pulse were a marked decrease in its frequency, and a remarkable gain in force and volume. None of the patients were advanced in life, forty being the highest age.—(*Lancet*, Aug. 27, 1870.)

Ergot.—(α). *Action and Uses of.*—It is generally accepted now that ergot acts conspicuously on unstriped muscular fibre, especially that of the minute arteries. Dr. Ch. Holmes (*Arch. de Phys.*, No. 3, 1870) has lately examined this question, and confirms the belief that ergot contracts the smaller arteries, and so increases blood pressure. Previous division of the vaso-motor nerves does not prevent the narrowing of the minute vessels, and this effect is also produced in the vessels of the lungs (*Journ. of Anat. and Phys.*, Nov., 1870). Brown-Séquard has shown that ergot and belladonna are the most powerful means of diminishing congestion of the spinal cord, and that ergot acts more upon the cord than belladonna. Both dilate the pupil, and they also diminish the reflex power of the cord.

(β). *Neuralgia.*—Dr. Woakes, believing that many cases of neuralgia depend on vaso-motor paralysis of the vessels of the skin and sentient nerves, was induced to try ergot. He gives five cases, ending in complete and rapid recovery, in which ergot was administered *per se*—viz., shingles, chronic sciatica, two cases of tic douloureux, and one of hemicrania. In one case of chronic and one of acute sciatica the ergot failed to relieve. Where ergot is likely to be useful its good effects commence immediately (*Brit. Med. Journ.*, Oct. 3, 1868). In the *Practitioner*, Oct., 1869, Dr.

Woakes resumes his advocacy of ergot as a remedy for neuralgia, especially in sciatica, and reports a case of shingles in which almost immediate relief was obtained from the severe neuralgic pains by 15 minim doses of ext. erg. liq.

(γ). *Other Affections.*—In *purpura* Dr. Bauer has used ergot with marked and speedy results; hæmorrhage is arrested, and the effused blood rapidly absorbed. Dose, 8 or 10 grs. of the powder one or more times a day. Relapses, however, are not prevented (*Brit. Med. Journ.*, May 22, 1869, from *Schmidt's Jahrbücher*, No. 1, 1869). Dr. H. Weber speaks highly of the gastric and subcutaneous administration of ergotin (aqueous extract) in *hæmoptysis;* and in the report for February, 1870, Art. 15, an account was given of Langenbeck's success with the hypodermic injection of ergot in *aneurism.* Lastly, Dr. Le Gendre has obtained cures in severe and long continued cases of *galactorrhœa* by the internal employment of ergotin, in doses of 2 or 3 grs. MM. Poyet and Commarmond had pointed out that the habitual use of bread containing a notable proportion of ergot of rye caused suppression of milk.—(*Rev. de Thér. Med. Chir.*, No. 2, 1870, from *Gaz. des Hop.*, 1869, No. 105.)

18. *Nitrite of Amyl.*—$C_5 H_{11} NO_2$. Mr. Umney has performed some experiments which show that much of the amyl nitrite now sold is in a most unsatisfatory state as regards *purity.* The nitrite should be prepared by passing nitrous acid into amylic alcohol (fonsel oil), which has been previously purified by fractional distillation till it owns a boiling point of 132° C. The purified nitrite should have a boiling point of 98°–99° C. Three specimens of amyl nitrite—sp. gr. 0·865, 0·874, and 0·856 respectively—were subjected to fractional distillation, and great differences were found in the products of distillation. The deviation from the direct boiling point indicated that, in the last specimen more especially, crude or merely rectified fousel oil was used for its preparation.—(*Pharm. Journ.*, Nov. 26, 1870.)

Of its *therapeutic* qualities we have as yet too little experience to guide us as to its precise powers for good or evil, but it is certainly an agent of remarkable power and rapidity of action against some forms of severe suffering, and it is noticeable, as an index of modern therapeutics, that the morbid conditions in which this drug has been found so serviceable were pointed out before it was used in practice. This compound ether, the homologue of the ordinary nitrous ether, was discovered by Balard, more fully investigated in

1859 by F. Guthrie, and again taken up and examined thoroughly by Dr. B. W. Richardson in 1863–64. Quite lately Mr. J. F. Goodhart has made some observations on its action on his own person chiefly. In one instance the optic disc was examined by Mr. Bader during the action of the drug, and the veins were seen to be dilated and varicose; arteries not materially affected (*Practitioner*, Jan., 1871). In the report for August, 1870, Art. 24, some reference was made to its physiological action, and to its power of controlling the terrible spasms of *angina pectoris*. Beside the cases communicated to the Clinical Society by Drs. Brunton and Anstie, Dr. Leishmann reports a case of angina in which its inhalation afforded relief (*Glasgow Med. Journ.*, Aug., 1869). In the *Edinburgh Med. Journ.*, July, 1870, Dr. J. Haddon relates three cases of the use of the nitrite in angina pectoris, and it is well to mark the alarming symptoms which occurred in two patients. In the first case a few sniffs of the drug invariably had the effect of almost instantly dispelling the paroxysm. In No. 2 the pulse was alarmingly slowed, the face did not become flushed, and he had a severe headache, but the original pain was not relieved. In No. 3 the face became flushed, the respirations were greatly reduced, and the patient was severely prostrated; the original pain was not assuaged. Each of these patients had valvular disease of the heart, attended with constant præcordial pain. Lastly, in the *Med. Times and Gaz.*, Aug. 6, 1870, there is a report of a severe case of frequently recurring angina in a woman, aged twenty-eight, which was relieved by Dr. H. Thompson with amylic nitrite, cautiously inhaled through one nostril for about ten seconds. Mr. Foster treated a case of *traumatic tetanus* in a man, aged fifty-two, with inhalations of five drops of the nitrite on each access of spasm; the spasms were immediately lessened, and the patient was convalescent at the end of a month. Mr. Foster remarks (*Lancet*, April 9, 1870) that, during thirty-four years of practice, he had seen seven cases of traumatic tetanus, all fatal until he tried the nitrite of amyl in this case.—(*Med. Times and Gaz.*, Sept. 3, 1870, a full summary of the subject.)

19. *Raw Meat in Diarrhœa and Dyspepsia.*—Dr. Druitt adds his testimony in favour of the efficacy of this mode of treatment. The meat must be thoroughly pulped by pounding or scraping, and then forms a soft pink pulp. It may be given by itself, or dusted over with white sugar, or diffused through a little strong beef-tea,

or made up into a stiff meat jelly, which is allowed to cool in a shape, and then eaten as a condiment. He especially recommends the raw meat in the acute and chronic diarrhœas of infants, and in the habitual diarrhœas associated with marasmus of strumous children, and in the obstinate vomiting of pregnancy, whether attended with diarrhœa or not.—(*Med. Times and Gaz.*, July 2, 1870.)

20. *Sal-ammoniac* (α) *in Nervous Affections.*—Dr. W. Cholmeley speaks most favourably of the utility of chloride of ammonium in nervous headaches, myalgia, and neuralgias connected with a depressed state of the system, and also in sciatica, lumbago, and the painful sequels of rheumatic fever. Moreover, Dr. Cholmeley considers it to have a powerful emmenagogue influence, especially when the amenorrhœa has occurred after exposure to wet and cold in delicate and nervous women.—(*Practit.*, Jan., 1871, from *Trans. St. Andr. Med. Grad. Assoc.*, Vol. iii.)

(β) *Hepatitis and Abscess of the Liver.*—Dr. Stewart is convinced that chloride of ammonium, in 20 gr. doses, morning and evening, is of signal value in cases of hepatitis when the acute symptoms have subsided and suppuration is either threatened or already established. Indeed he is sanguine enough to regard it as a specific in these diseases, and claims powers for it which the evidence adduced scarcely seems to sustain.—(Pamphlet by W. Stewart, M.D., Surgeon 21st Fusiliers: Rangoon, 1870.)

21. *Sarsaparilla in Syphilis.*—Dr. Clifford Allbutt speaks most decidedly, and after long experience, in favour of large doses of sarsaparilla in tertiary and visceral syphilis. He administers from four to ten ounces of the decoction three times a day, *i.e.*, a pint or a pint and a half during the twenty-four hours. He urges its value in cachectic cases attended with sluggish ulcerations and indolent gummata, especially when mercurial preparations or iodide of potassium have failed, or where they are on some grounds to be avoided.—(*Practitioner*, May, 1870.)

CHLORAL.

Tests for Purity of.—It was pointed out in the last Report that, by a departure from the original process of Dumas and Liebig for the preparation of chloral, a compound has been introduced into commerce which is not pure chloral, but consists of chloral + a

molecule of alcohol. This alcoholate of chloral is offered for sale as hydrate at a much lower rate than the market value of true hydrate of chloral, and it is highly important to be able to distinguish it from real chloral hydrate, not only because it yields upon decomposition a smaller quantity of chloroform, but also because its physiological effects, according to Liebreich, differ essentially from those of the hydrate. Drs. C. A. Martius and P. Mendelssohn-Bartholdy, who carry out the manufacture of chloral under the direct supervision of Dr. Liebreich, have shown (*Pharm. Journ.*, July 30, 1870, from Buchner's *Repertorium für Pharmacie*, 1870) that chloral combines not only with ethylic alcohol, but also with the other alcohols of the fatty series, *e.g.*, methylic, butylic, or amylic alcohol, and with mercaptan. Chloral does not combine with the ethers of the alcohol radicles. The methylic compound boils at 98°C, and closely resembles the ethylic compound. The amylic compound boils at 143°C, and at 25°C has a sp. gr. of 1·234. The ethylate (alcoholate) boils at 116°C, and solidifies at 40°C, becoming crystalline. At 40°C (in the fluid condition), its sp. gr. is 1·143. Pure hydrate of chloral boils at 95°C. Alcoholate of chloral is much less soluble in water than the hydrate. In addition to these tests of solubility and boiling point, Mr. C. Umney indicates a simple quantitative test for determining the chloroform value of any specimen of chloral hydrate.—(*Pharm. Journ.*, Aug. 6, 1870.)

(α). *Hydrate of Chloral.*—Dissolve 500 grs. in about 1 ounce of distilled water; transfer to a 1,000-grain graduated tube, and make up to 700 grain-measures; to this add solution of ammonia (0·891) until the whole measures 1,000 grs. Agitate, immerse the tube in warm water, and set aside for 12 hours. The volume of the chloroform layer should not be less than 235 grain-measures, which (sp. gr. chloroform 1·497) would give 351·7 grs. by weight = 70·3 per cent. of the chloral employed.

(β). *Alcoholate of Chloral.*—Proceed as in testing the hydrate. The chloroform layer will be about 200 grain-measures, or by weight 299 grs. = 59·8 per cent. of the chloral alcoholate. Hence chloral hydrate yields 10 per cent. more chloroform than chloral alcoholate.

Antagonism to Strychnia.—According to the observations of Liebreich, since confirmed by Dr. Hughes Bennett, it appeared that the action of chloral interferes with that of strychnia. Dr.

Rajewsky has also investigated this question (*Centralbl. f. med. Wissensch.* 17, 1870), and has arrived at the following conclusions: 1. Small doses of chloral hydrate can prevent strychnia cramps, and can annul them when already established. Even when a quantity of strychnia, considerably beyond a fatal dose, is taken, life can be maintained by means of chloral, especially if artificial respiration be kept up till the chloral is absorbed. 2. After the introduction of mortal doses of chloral life cannot be maintained by the aid of strychnia, although the lost reflex excitability be restored. Death occurs by progressive failure of the respirations, and after the administration of large doses of strychnia tetanus even sometimes sets in. 3. After the introduction of strychnia and large doses of chloral the extreme susceptibility to tactile irritations continues, notwithstanding the diminished reflex excitability. As an antidote to strychnia it is better to administer chloral in but moderate doses.—(*Wochenbl. der k. k. Gesellsch. d. Aerzte in Wien*, June 8, 1870.)

Effects of Large Doses.—A "sleepless correspondent" of the *Lancet*, Aug. 13, 1870, was in the habit of taking chloral in place of morphia for months with the happiest effects. He was obliged to gradually increase the dose, and latterly ʒi. was taken without benefit. On the 2nd of August he took ʒi. of chloral hydrate at 11.30 p.m., and soon after went to bed. At 1 a.m. he repeated the dose, but no hypnosis following, he got up and swallowed another full dose. He was scarcely in bed till he was fast asleep, and slept soundly for eight hours. When he tried to get out of bed in the morning he found to his horror that he had no control whatever over his legs from the knees downwards, and was quite unable to stand; no other muscles were paralysed. After a hearty breakfast he was soon able to walk as usual, except for a slight unsteadiness of gait and some diminution of sensation in the lower extremities. In all, between the hours of 11 and 2, he swallowed about 7 *drachms* of chloral. Dr. G. Dobbs prescribed 30 grs. of chloral for a pregnant lady suffering from distressing asthmatic breathing. At midnight, four hours after the dose, she awoke, and was given by the nurse 20 grs. more, and at 12.30 a.m., being restless, she insisted on having another dose. About 2 a.m. she suddenly sat bolt upright in the bed, complained of agonizing cramps in the legs, of a feeling of suffocation, swimming in the head, and of absolute inability to co-ordinate the movements of the lower extremities. Consciousness was not disturbed. The face was

intensely flushed, eyelids closed, conjunctivæ injected, and pulse for some time at 120, bounding. She recovered in about 16 hours, but had lost all recollection of events after swallowing the second dose at midnight. Her sensations were those of unlimited rest, yet quite different from sleep. A hippo emetic was the only treatment adopted (*Med. Times and Gaz.*, Oct. 8, 1870). Compare Dr. J. R. Reynolds' case, *Dub. Quar. Journ.*, Aug., 1870, p. 193. The following case, occurring at the Philadelphia Hospital, is reported in the *Canada Lancet.* Mrs. B., a nurse who had been up all night attending a patient in delirium tremens, took at a.m., for the relief of headache, a solution of chloral which she found in the ward. When visited half an hour after, she was in a profound stupor, with stertorous respiration, pulse small, 140. Mustard was applied to the extremities, and vigorous flagellation resorted to for an hour before there was the least sign of returning consciousness. Faradisation was directed on the phrenic nerve; she gradually came to, and after a long sleep, occasionally interrupted for the purpose of giving nourishment, she awoke next morning quite comfortable, except for a slight headache. From the strength of the chloral solution and the quantity known to have been in the bottle, she is presumed to have swallowed *at least* 460 *grs.* of the hydrate (*Med. Press and Circ.*, Jan., 1871). A *fatal* case of poisoning by chloral is recorded by Mr. J. F. Brown. A lady, who suffered greatly from want of rest, swallowed nearly one ounce of chloral hydrate, felt ill almost immediately, and expired within an hour.—(*Pharm. Jour.*, July 2, 1870.)

Modes of Administration.—The hypodermic injection of chloral seems to be quite abandoned, and there is nothing that calls for particular remark. Different firms, with the desirable intention of concealing the unpleasant taste of chloral, have brought out various preparations under the names of "aromatized syrup," "spiritus chloralis," "syrup of chloral hydrate," &c., which will, no doubt, be acceptable to many individuals. In prescribing it care must be taken to distinguish it from *chloralum*, the new antiseptic, which has been so unfortunately named.

Therapeutic Uses.—In conformity with the preceding reports chloral will be noticed in its application to practice as a means of relaxing muscular spasm, of assuaging pain, and as a nervous sedative and hypnotic.

1. *Muscular Spasm.*—In the *Lancet*, Sept. 24, 1870, three additional cases of *tetanus* are recorded which were subjected to

treatment by chloral. In the two cases of traumatic tetanus the sufferings of the patients were greatly mitigated, and both ended in recovery. In the third, a case of idiopathic tetanus, half an ounce of chloral administered within ten hours had no effect in causing relaxation of the jaws, which, however, was brought about by the inhalation of a little chloroform. Mr. Lawson Tait records three cases of tetanus, all ending fatally, in which he severally used wourali, calabar bean, and chloral. The first drug produced no good effect, and perhaps even hastened death. The calabar bean proved equally useless, and the chloral procured sleep, and so saved suffering, but did not seem to delay the fatal issue.—(*Lancet*, Oct. 1, 1870.)

Since the recommendation of chloral in tetanus by Liebreich it has been used by Dr. Ballantyne, M. Verneuil, Mr. Birkett, Mr. S. Watson, and others, yet the conclusion of Dr. Cowling, founded on an analysis of 415 cases, that "when the symptoms last 14 days recovery is the rule and death the exception, apparently independent of the treatment," renders us slow in admitting at present the therapeutic value of chloral as of any peculiar value in the treatment of this formidable disease.—(*New York Med. Journ.*, Nov., 1870.)

2. *Pain.*—Mr. Weedon Cooke feels bound to add his testimony in confirmation of the value of chloral in *cancer*. As a rule, 20 grs. are quite sufficient for a night draught, but, when pain is persistent, 10 grs. three times a day gives the greatest satisfaction. Occasionally the combination with morphia has proved beneficial (*Med. Times and Gazette*, Dec. 31, 1870). Mr. E. Lambert has published, in the *Edinb. Med. Journ.* for August, 1870, an account of 11 cases of labour in which chloral was administered. Chloral is an agent of great value in the relief of pain during *parturition*, and not only does not suspend, but rather promotes uterine contraction by suspending all irritating reflex actions.—(*Brit. Med. Jour.*, Oct. 1, 1870.)

3. *Nervous Sedative and Hypnotic.*—The purely hypnotic powers of chloral need no further confirmation, for they have been attested from every quarter of the world, and its sedative effects in many distressing maladies of the nervous system receive daily witness in their favour. Dr. W. Strange urges its efficacy in the treatment of *nervous* and *cardiac disorders*, attended with restlessness, anxiety, and want of sleep, and he points out that it is not truly an anodyne except in so far as pain is removed while sleep continues (*Med.*

Times and Gazette, Sept. 17, 24, 1870). Dr. Habershon's practice has taught him caution against the indiscriminate use of chloral, for he believes that chloral has a tendency to produce congestion of the bronchial tubes and lungs, and that it is not a suitable medicine in cases where the respiration is liable to embarrassment (*Lancet*, Sept. 17, 1870). Further evidence as to its utility in *delirium tremens* is forthcoming, and Mr. Lansdown and Mr. Corbet Fletcher each record cases showing the immediate good effects of chloral in this disease. Dr. Macleod has administered chloral in various forms of *insanity*, *e.g.*, paralysis of the insane, periodical mania, melancholia, and dementia, and speaks in the highest terms of its value as a nerve-sedative independently of its hypnotic action. The patients gained in weight and strength, the action of the bowels and bladder was improved, and in no case was there caused any dislike to food, but rather the contrary (*Practitioner*, Aug., 1870). Mr. Wadsworth is similarly pleased with chloral in cases of mania, and finds it convenient to administer the drug in porter. (*Brit. Med. Jour.*, July 16, 1870). Lastly, Dr. J. Russell, of Glasgow, has published some "Clinical Observations on Hydrate of Chloral as a hypnotic in *typhus*," which exemplify the uniform and rapid success of chloral in calming the different forms of cerebral excitement incident to fever. For fuller details we must refer our readers to Dr. Russell's interesting pamphlet.

PART IV.

MEDICAL MISCELLANY.

Reports, Translations, and Scientific Intelligence.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

Dr. J. Stannus Hughes, President.

Pneumonia.—Dr. Hayden submitted to the Society a case of acute pneumonia, a disease now so prevalent, and, he might add, so fatal, among the poor of Dublin. On the 14th of this month, a young and previously healthy woman, twenty years of age, was admitted to the Mater Misericordiæ Hospital. She was suffering at the time from most intense respiratory distress. Her face was congested; her pulse 144; her respiration 60 in a minute. The skin was burning hot. She coughed incessantly, but did not expectorate; and she moaned likewise constantly. A cursory examination of the chest was sufficient to show that she was suffering from pneumonia, engaging the upper and middle lobes of the right lung. She was put under the most active treatment immediately, which failed, however, to make more than a transient impression upon the disease. After a couple of days the left lung became engaged, and the upper lobe of that lung only, as on the right side. She sank on the fifth day. On examining the chest, it was found that the upper lobes of both lungs were solidified. There had been likewise pleuritis; there were some false membranes visible upon the outer surface of both lungs. The inferior lobes of both lungs were unaffected. It was interesting as illustrative of the insulating property which the fissures of the lung possess in regard to the extension of acute inflammation. In the apex of the left lung several tubercular cicatrices were found, beneath which and surrounded by condensed fibroid tissue, two or three minute vomicæ were discovered, filled with thick purulent matter. The apex was more or less contracted, and firmly attached to the chest wall by old adhesions. He desired to ascertain whether, by means of microscopic examination, anything might be discovered tending to explain the rapidly fatal character of the disease—

the woman being only twenty years of age, five days ill, and having previously enjoyed good health. He had, therefore, made a careful examination with the microscope, but it showed only vascular engorgement of the pulmonary tissues and excess of epithelium. Everywhere the air passages seemed to be free. The upper portion of the right lung, the one first attacked—was not in so advanced a state of disease as the upper portion of the left. From this he inferred that resolution had commenced on the right side when the left side was attacked, and the engorgement of both lungs at the same time, must have been the cause of death. For the last few weeks he had been scarcely a day without three or four cases of pneumonia under his care in hospital, and several of these had been fatal. Probably the rapid changes of the weather from hot to cold, and from dry to moist, might serve in some measure to explain this.—*November 26th*, 1870.

Fracture of the Spine.—Dr. M'Donnell said he wished to exhibit a specimen, and briefly detail the particulars of an injury to the spine, of a rather remarkable and rare character. The patient, James Byrne, was a labourer, aged forty. He was admitted to Steevens's Hospital on the 31st of May. He had been sent up from Dunboyne, where he had been engaged in putting some hay on a cart, and he accidentally fell off, coming down on the back of his head on a dung heap. He was not stunned, but was immediately paralysed in the arms and lower limbs. When he arrived at Steevens's Hospital, he (Dr. M'Donnell) thought the case one about which he should seek the advice of his colleagues in consultation. They made a most careful examination, but were unable to find any evidence to indicate the place where the fracture of the spine had occurred. At the same time there was no doubt whatever, from the symptoms, that there was an injury of the spine or of the spinal marrow high up. The lower limbs were absolutely paralysed as regards both motion and sensation. The upper limbs were paralysed as regards motion completely, and as regards sensation from the middle of the arm on each side. In fact, there was no sensibility anywhere below the level of the nipples. He was incapable of passing water, and had all the symptoms of fracture of the spine. They made a most careful examination, and felt what appeared to be a displacement about the fifth or sixth cervical vertebræ, but there was no sufficient evidence of it or of pressure to justify any operative proceeding. The patient went on from bad to worse, but lived for two months after the date of his admission to hospital. During all this period he was carefully watched, and some of the symptoms were of special interest. He (Dr. M'Donnell) had for some time past been greatly interested in one phenomenon which presented itself in cases of injury of the spine, viz., the changes of temperature. It was known that in some of these cases

there was a remarkable increase of temperature. Within the last eighteen months two cases were admitted into Steevens's Hospital, one under the care of Dr. E. Hamilton, and the other under his (Dr. M'Donnell's) care, in which an increase of temperature was observed. In one case the thermometer stood at 106°, and in the other at 104° in the axilla. This subsided, not continuing, as was sometimes the case, until the period of death. In one remarkable case recorded by Sir Benjamin Brodie, the thermometer stood at 111° in the groin, and even a little higher after death. This subject had interested him (Dr. M'Donnell) a good deal. He therefore directed this case to be carefully noted. The pulse and temperature were accurately noted, and the highest point at which the thermometer ever stood was 99·6. It was generally 98. The pulse was slow during the early days, being not more than from 50 to 60; but as the case went on and bed sores were produced, it became somewhat higher. The symptoms indicative of paralysis of motion were complete. Those having reference to sensation were complete from the nipples down as regards the trunk and lower limbs, and from about the middle of the arms as regards the upper limbs; but the left arm underwent some improvement, and he was, after a time, able to feel when the fingers of the hand were pinched. In this case reflex symptoms were exceedingly well marked. Tickling the soles of the feet caused active spasmodic twitching; but in one respect he never remembered to have seen (except in the case of a pithed frog) such remarkable reflex phenomena as were exhibited in this case. They knew that in the case of a decapitated frog, after the head had been completely taken off, if a drop of acetic acid be dropped on the body the headless animal will make a reflex movement, so admirably conducted that it puts the foot up and brushes away the irritant from its side; and if you hold one foot it will throw up the other and try to put it away; so that a casual observer would not think the animal was dead. They rarely saw anything of this kind in the human being; yet in this case it was exceedingly well marked. Over and over again, when introducing an instrument into the bladder, although the man was absolutely unconscious of the operation, the left arm would wriggle up and down, and the hand come fluttering over the genital organs. Even when the eyes were shut, and the hand held over them, so that he did not know the catheter was being introduced, the left arm, which was the one most in relation with the sensorium, moved violently and came down over the pubis. He never remembered to have seen any case in the human subject in which that feature was so well marked. The man also had priapism for a few days, but this symptom soon subsided.

They must consider that a patient who had sustained a serious injury of the spinal marrow in the region of the neck, lasted a long time if he held out for two months. A week or ten days was the time such cases

generally ran, and, curiously enough, they generally died very suddenly. Although two months was a long period for this case to run, there were several cases on record which had lasted much longer. Mr. Page, of Carlisle, gave a case of fracture of the fourth and fifth cervical vertebræ where the spinal marrow was completely severed and where the patient lived fifteen months. He always referred to this case as a striking one, because he thought where the spinal marrow had been cleanly cut through there was much less irritation, and bed sores formed less frequently than where the spinal marrow was lacerated, or a spicula of bone imbedded in it, keeping up constant irritation. The cases that run most rapidly into bed sores, cystitis, &c., are those where from some cause there is prolonged irritation. Another case is mentioned by Mr. Hilton, where a man lived fourteen years, although completely paralysed in the bladder, legs, and rectum. Mr. Phillips gave a case where the patient lived fourteen weeks, although the fracture was so highly placed as the atlas. He (Dr. M'Donnell) showed Prof. R. W. Smith a remarkable case, in which displacement occurred between the atlas and the axis. The bones were lent to him by a gentleman in London, and Professor Smith carefully examined them. The atlas was pressed over in such a manner that there was an opening between the back part of the atlas and the axis, into which the little finger only could go, so that the passage through which the medulla oblongata goes was narrowed, and the odontoid process was fractured. The man not only lived but wheeled a barrow a mile after receiving the injury. He was wheeling a barrow with one of those wooden frames on his neck which milkmen wear in London, and something struck him, so that the wooden frame was pressed down on his neck, causing the injury. He recovered from the shock, and, holding his head in his two hands, he went back for his barrow and wheeled it a mile.

These must be regarded as very exceptional cases indeed, but they showed that we must not wonder too much at such a case as the present, where the patient lived for two months after receiving a fatal injury. The remarkable feature of the present case was that there was no pressure on the spinal cord, and the spinal cord almost seemed to have been fractured itself. On viewing the vertebræ in profile, there was a remarkable indentation observable, which he could run his finger along. He could not discover that there was any fracture, but the bodies of the two vertebræ were anchylosed firmly. The laminæ and oblique process were firmly anchylosed; nevertheless, the spinal canal was not narrowed. He removed the chord and placed it in spirits, intending to make a section of it when it was quite hardened. One remarkable circumstance, as a matter of coincidence, was worthy of attention. As he had mentioned, when examining the back of the neck they observed something like an irregularity, and one of the spinous processes on the cervical vertebræ was

curiously bifurcated. One of the spines was longer than the other, so that on examining it, one would think that the congenital malformation was a displacement; nevertheless, although it occupied the place where the injury probably took place, it did not beguile them into trephining the patient. Dr. M'Donnell exhibited the chord, and pointed out that there was no trace of any blood clot or discolouration, but there was a remarkable indentation. He might almost say it was fractured; it was quite limber, and there was a reticular structure, which no doubt was the remains of some little blood clot poured out at the time, and which, in the lapse of time, became imperfectly organized; but there was no attempt at the formation of nervous tissue. The anterior columns had escaped, but as regards the posterior columns, the entire of the grey structure had been divided—*November 26th*, 1870.

Stricture of the Œsophagus.—Dr. Barton said the specimen which he now submitted possessed a double interest—in the first place a pathological interest, and in the second place a clinical one. It was a case of scirrhus, affecting the œsophagus. They would observe in the specimen a mass of scirrhus disease occupying the central portion of the œsophagus, commencing where that tube is passing down from the neck and occupying its coats where it lies in the posterior mediastinum. This extensive mass of morbid deposit had passed on to the stage of softening. The tube of the œsophagus was completely enclosed in it, and everything that passed into the œsophagus must have passed over or into this extensive diseased mass. The lower portion of it was distinctly marked by a clear, well-defined line, below which the smooth, pink membrane of the œsophagus was seen to be completely unaffected. The disease appeared to have commenced in the œsophagus and extended to the glands in the upper part of the thorax. The rough distinct line where the disease terminated, the ulcerated portion occupying the middle of the œsophagus, and the healthy appearance of the mucous membrane of the epiglottis, the larynx, and the mouth, were the chief pathological features of the case. The clinical features of it were also interesting. The patient was admitted to the Adelaide Hospital last May. He came from Arklow, where he had been exposed to great vicissitudes of weather, being constantly out in a fishing boat exposed to cold and wet. In the previous December he noticed a hoarseness. He had some little expectoration and cough, but the chief thing that attracted his attention was the hoarseness. This increased, and he found one morning, on getting up, that he had completely lost his voice. This alarmed him. He consulted a surgeon; and, as he appeared to be getting worse, he came up to hospital. Upon admission his voice was found to be almost gone. He spoke in a hoarse whisper, above which he could not by any effort raise his voice. He complained of a fixed pain, which he referred to the region of the larynx and to the

scapula behind. He had no pain lower down than the thyroid cartilage. He complained of great difficulty of swallowing. He could not swallow any solid food, and could only take fluids with difficulty and with some pain; but he never referred any pain below the region of the epiglottis. The lips were of the natural hue, so that there evidently was no obstruction to respiration. The respiration was not very rapid. The husky, deep, and almost inaudible tones of the voice continued. A most careful examination of his chest showed merely that there was a very slight increase of respiratory murmur, and resonance in the apex of the left lung; but low down in the neck, on the left side, a small hard gland was perceptible. The diagnosis arrived at was that there was probably malignant disease, and that it occupied the interval between the pharynx and the larynx—that it occupied the pharynx and pressed upon the larynx. There was at that time a woman in the hospital who had undergone the operation of tracheotomy and recovered from it. The poor man was eager that the same operation should be performed on him, exclaiming that he was dying from difficulty of breathing; but as it was evident his distress arose from a different cause than that which existed in the case of the woman, his request had, of course, to be refused, and he was allowed to go on without any further interference. He died on the 16th of July. He made a request, rather unusual for persons in his condition of life, that he (Dr. Barton) should make a careful examination of his body. The clinical interest of the case was apparent. All the symptoms were referred to the larynx, and yet it was absolutely free from disease. The pharynx was unaffected.—*November 26th*, 1870.

Tuberculosis.—Dr. Little said the morbid specimen before the Society tended to confirm certain views recently put forward on the subject of tubercle; from the nature of the case it was not possible in any individual instance to demonstrate the correctness of these views, our conviction of their truth or the reverse must arise from the examination of a number of specimens which in a greater or less degree lent probability to the new doctrine.

It had some years ago been ascertained that certain animals readily became tuberculous; in them inoculation with morbid products or the infliction of an injury was found after some weeks to be followed in a certain number of cases by the growth of tubercles in the lungs, in the liver, and in the spleen; further observation showed that in the animals which escaped tuberculosis after these experiments, the wounds had healed kindly, whereas in those which became infected there had been produced at the seat of the injury or inoculation a mass of caseous induration.

This fact taken in connexion with Buhl's discovery that in a vast number of persons who died of tubercle, old caseous formations were

found in the body, had suggested the idea that tuberculous growths had their origin in the absorption from a diseased gland, or from some other caseous mass in the body, of its corpuscular elements, which thus found their way into the blood and caused after a certain time the growth of tubercle.

The abdominal and thoracic viscera he now presented were taken from a boy aged four years, who in August had been admitted to the Adelaide Hospital by his colleague, Dr. W. G. Smith, and who subsequently passed under his (Dr. Little's) care. The symptoms on admission were those of infantile remittent fever, and it was noticed that the cervical and inguinal glands were enlarged; a few days after his admission the fever subsided, but he soon afterwards exhibited the symptoms and signs of double bronchitis; he lost flesh, had thirst, excessive appetite, looseness of the bowels, and swelling of the abdomen, and was in a semi-comatose state for a fortnight before his death, which took place two months after admission. The spleen presented exactly the appearances seen in the spleens of guinea-pigs after inoculation, the surface of the liver and both lungs were studded with tubercles, but besides these tuberculous growths there was found an immense mass of old diseased bronchial glands, surrounding the bronchial tubes, and the deep cervical glands had undergone a process of cheesy degeneration. He thought the specimen worth showing, as one of those cases that tended to establish the truth of the modern view of the origin of tubercle.—*November 26th*, 1870.

Intractable Diarrhœa connected with Duodenitis and Fatty Liver.—Dr. Little said the man from whom the liver and stomach he now exhibited were taken was thirty-two years of age—a fine, well-made man—a druggist's assistant. His habits had been very intemperate, but he enjoyed good health until the middle of August last, when one night he was suddenly attacked with violent pain in the belly and profuse sweating. The following night he had a similar attack, and from this time he suffered from looseness of the bowels. About a month after the commencement of his illness he came under his (Dr. Little's) care in the hospital. His tongue was furred, he had little desire for food, and his bowels were moved six or seven times in the twenty-four hours, the stools consisting of brown liquid fecal matter. The abdomen was soft and neither distended nor retracted.

It was noticed that all over it the percussion sound was dull. He acknowledged to a little tenderness about the margin of the liver, and here the rectus muscle was abnormally tense. He lived for two months, during which there was very little change in his symptoms—furred tongue, little desire for food, persistent looseness of the bowels, and loss of flesh.

The pulse kept very uniformly at 100, and the temperature was not

elevated. The urine seldom exceeded twenty ounces in twenty-four hours; it was of a deep colour and had a density of 1030; on the addition of nitric acid an abundant sediment was produced which, on microscopic examination, proved to be crystalline. It had long been known that in testing urine it was not justifiable to conclude that albumen was present because cloudiness resulted from the addition of nitric acid, as in some specimens of urine a precipitate of uric acid, or nitrate of urea was immediately produced by the addition of nitric acid; this phenomenon had been considered of little importance, except in so far as it constituted a source of fallacy in the search for albumen. Dr. Beale, however, had noticed that it occurred in some cases where a rapid change was taking place in the structure of the liver, and he (Dr. Little) was inclined to think it might have some diagnostic value. In the present case the crystals were those of nitrate of urea.

During the last fortnight of life the patient had much vomiting, and passed from the bowels rather fluid blood in considerable quantity. The body was examined twenty hours after death; it was much emaciated, but on opening the abdomen the great omentum was seen to be unusually ample and loaded with fat, the kidneys were embedded in fat, and all the mesenteric folds were thick and greasy.

On reflecting the abdominal wall just below the margin of the liver he found a sloughy mass, out of which there oozed a grumous fluid; the parts were so softened that there was some difficulty in ascertaining the source of this fluid, but a careful separation of them showed that it came from the duodenum, in which a perforation had occurred, just outside the pylorus. Beyond this the intestine exhibited nothing abnormal, except lividity of the mucous coat.

The stomach had two perforations; he could not say whether these had been produced during its removal from the body; if so there must have been considerable previous softening, for the removal was effected with great care. There was complete erosion of the mucous membrane over three patches in the stomach; whether or not these were due to *post-mortem* softening he could not say; but if they were, it was evident that the coats of the stomach and the duodenum must have undergone a change during life to render them liable to softening in so short a time after death. The liver weighed ninety-six ounces, its margin was rounded. It was pale, a great deal softer than the healthy organ, and a section of it communicated a greasy stain to blotting paper, and burned rapidly.

From this case we learn that there may be persistent diarrhœa associated with disease high up in the intestinal tract, and considerable disease of the duodenum and stomach without much vomiting. The principal reason, however, he had for exhibiting the specimen was on account of the phenomenon presented by the urine in this case, in which

it was evident a rapid process of disorganization had been going on in the liver, just as had been the case in some of the instances referred to by Dr. Beale.—*November 26th*, 1870.

Hepatic Abscess.—Dr. W. Moore exhibited a specimen of abscess of the liver, which, he observed, was a comparatively rare disease in this country. The history of the case was as follows :—A man was admitted to Sir Patrick Dun's Hospital on the 19th of October, complaining of a violent pain in his right side. He was about forty years of age, a powerful-looking man, had been a labourer all his life, and was of temperate habits. He had a very severe diarrhœa for a fortnight before he came into hospital; but on the day previous to his entering the hospital the diarrhœa had stopped, and he was seized with a violent pain in the right side. He also had shivering fits for the twenty-four hours previous to his admission. After admission the shivering fits returned, and the violent pain in the right side. Leeching over the side and hot fomentations were adopted. Still the pain went on, and he had an occasional rigor, with fever, a quick pulse, thirst, and a tongue inclined to be morbidly red. About the end of four days (on the 24th) a slight enlargement became visible over the right side. It became more perceptible on the 26th, and looked like matter beginning to point. It was apparently a small flattened tumour. A careful examination was made; and taking into account the rigors, the conclusion arrived at was that matter was pointing. The tumour was more perceptible on the 27th and 28th, and the pointing seemed to be more decided. The man's fever increased; he had great vomiting; his tongue was of a dark red colour, dry and glazed; very quick pulse, and intense thirst. It was decided to try tapping, and on the 31st of October Dr. Bennett made an opening, from which a certain amount of matter came away, probably altogether about half a pint. The issue of matter was not commensurate in amount with the size of the tumour; however, the man expressed himself as having been much relieved by the operation; but on the next day there was no diminution in the size of the tumour, and the insertion of a probe to its full length did not assist the escape of matter. Next day the tumour became still more enlarged, and filled the epigastric, the umbilical, and the right lumbar region. Vomiting continued, and hiccough supervened; and on the 6th of November the man sank. The *post-mortem* examination was most carefully made by Dr. Bennett. Upon opening the abdomen there were found considerable adhesions in the hepatic region; and on coming down on the liver there was a large cavity, which contained about two quarts of pus. It engaged the inferior half of the right lobe of the liver, burrowing more towards the left lobe, and measured, in every direction, about five and a-half inches. Therefore, the capacity of this abscess was very considerable. There were strong hepatic bands running

through it, which, no doubt, prevented the great issue of matter which should have taken place. The cavity was also lined with a thick, strong fibrinous membrane, although of comparatively recent formation. Dr. Moore thought the case worth bringing before the Society for various reasons. In the first place, one might naturally have expected jaundice to have been a symptom in the case; but experience has shown that jaundice is a comparatively rare symptom in hepatitis, Broussais having observed it twenty-three times in sixty-six cases; while Louis only saw it twenty-six times in 155 cases. In this case there was no direct pressure on the bile ducts, nor did ascites exist, as the portal vein was not interfered with, nor was there pulsation in the tumour (although it overlay the abdominal aorta), which may be accounted for by the fact that the fluid was present in such quantity as to prevent any pulsation being transmitted through it. There was no dyspnœa, as the free edge of the liver was the seat of the abscess, the weight of which tended rather to relieve than compress the lung. There was no look of "malignant cachexia" about the patient, and the diagnosis of the disease was arrived at by the accession of rigors, followed by the flattened tumour in the hypochondriac region.—*December 3rd*, 1870.

Cancer of the Stomach.—Dr. Grimshaw exhibited the stomach, removed from a man, aged fifty-five years, who was admitted into Steevens's Hospital on the 1st of November, 1870, and died on December 1st. The patient did not give a very clear account of his previous history. He stated he had vomited occasionally during the last year and a-half, but had continued to follow his occupation (which was that of a field labourer) until about a month before admission, but had to give up in consequence of weakness, pain in the stomach, and loss of flesh. On examination, Dr. Grimshaw found a tumour occupying the epigastric region, and filling up the angle formed between the cartilages of the ribs below the xiphoid cartilage. The tumour was very hard to the feel, and slightly painful on pressure. The patient did not complain of the tumour, and seemed to think it of little importance. His chief complaint was of vomiting, which frequently occurred shortly after taking food. On percussion over the abdomen the area of gastric clearness was more extensive than normal, showing dilatation of the stomach. The bowels were usually confined, and when moved the discharges were scanty. Dr. Grimshaw came to the conclusion that the case was one of cancer of the pyloric orifice of the stomach. For the first few days the stomach became less irritable, and otherwise matters remained much the same as on admission. The tumour then moved downwards, and gradually passed downwards and to the right side, occupying the right hypochondrium. The stomach became much more distended, and the vomiting became worse than before. The motion of the tumour, together with the distension of the

stomach, confirmed Dr. Grimshaw in the diagnosis he had arrived at, as their motion has been shown to be a sign of pyloric tumours, unconnected with neighbouring organs. The patient's bowels now became quite inactive, and purgatives, given by the mouth, had no effect. He died on the 1st instant. On opening the abdomen a large quantity of fluid escaped, and on first appearance the stomach *seemed* to occupy the whole abdominal cavity. The stomach, when removed, was found to measure thirty-seven inches in circumference around the great curvature. The pyloric orifice was involved in a large mass of cancerous deposit, engaging the whole of the pyloric extremity of the stomach and the commencement of the duodenum. A few scattered nodules of this cancerous material were found in the great fissure of the liver, and a few scattered among the abdominal glands. On opening the stomach a large quantity of dark-brown fluid escaped. The internal surface of the tumour was ulcerated and fungoid, occupying the parts already mentioned. On microscopical examination the tumour was found to consist of large circular cells, filled with a fine granular matter, and imbedded in a fibrous stroma; there were also some large oat-shaped nucleated cells, but they were not as numerous as the smaller cells with granular contents. The tumour, therefore, presented the structure usually associated with malignant growths.

Dr. Grimshaw pointed out that the gradual movement of a tumour of this sort was an important diagnostic sign, especially when associated with distension of the stomach.—*December 3rd*, 1870.

Caries of the Temporal Bone.—Dr. Bennett presented to the Society two specimens of disease of the temporal bone, which had been induced by otorrhœa. The cases were in many points similar. The patients were both women of from thirty-five to forty years of age. Both had been healthy up to a short time before their death, with the one exception, that for about a year or a year and a-half in each case there had existed occasionally discharges from the ear.

The first patient was admitted to Sir P. Dun's Hospital in the beginning of September. For some seven or eight days previous to admission she had been ill, had been much excited, and had complained of violent pain in the head. On admission she was comatose. The diagnosis of the case was difficult, as there was no accurate history to be obtained, nor were there present any diagnostic signs of fever, nor any absolute evidence of encephalitis; nothing pointed to the existence of intracranial inflammation except the presence of a slight discharge from the ear and the vague signs of pain in the head. The patient's hand wandered frequently over the side of her head, but there was neither redness nor swelling to be detected in the neighbourhood of the ear. After death the pathological changes were found to be, acute disease of the petrous portion of the temporal bone, and inflammation of the pia mater. The

external meatus contained a small amount of discharge, but the membrana tympani, though opaque and thickened, was not perforated. The tympanum was filled with inspissated mucus, and the bone was softened and red, but there was no purulent matter visible. The bone was so soft that a knife could be easily sunk into its tissue. The dura mater on the surface of the petrous bone was slightly vascular, but its arachnoid surface was normal, except for a very limited space around the meatus internus. The subarachnoid spaces at the base of the brain were filled with yellow lymph, distributed in the same manner and of the same colour, &c., as the lymph produced in epidemic cerebro-spinal arachnitis. The lymph was deposited in greater quantity on the side of the brain corresponding to the diseased bone, and around the exit of the seventh pair of nerves on that side. In this position the parts of the brain adjoining the nerve, and the nerve itself, were indurated. The portio mollis was adherent to the bone in the meatus internus, and there were present around the lower part of the opening adhesions of old date between the layers of the arachnoid. The inflammation had apparently been propagated from the diseased bone along the tissues investing the nerve, and had so reached the subarachnoid space. There was evidence of at least one previous attack of inflammation affecting the same parts in the indurated condition of the nerve and parts of the brain around its exit and in its adhesion to the sides of the meatus. There was not at any time paralysis of the portio dura on the affected side, but the patient had been deaf for some time.

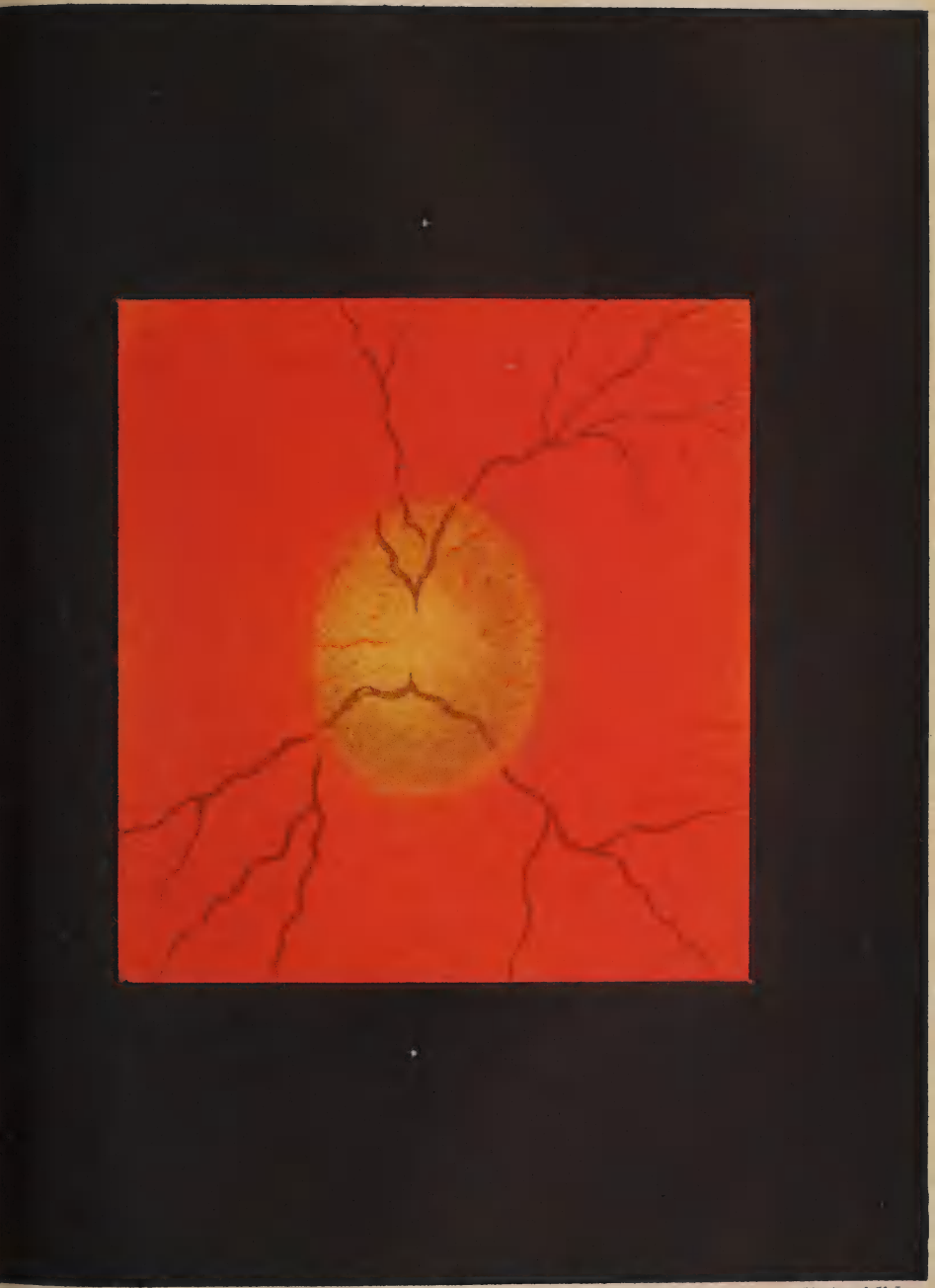
The second patient was also admitted during the month of September to Sir P. Dun's Hospital. About a fortnight before her admission she suffered severely from pain after syringing her ear for the relief of purulent discharge, which had existed for some time. She became ill, and was confined to bed for some days before her admission to hospital. On admission she was conscious. She had great pain in her head and ear, and had a very rapid pulse and very hurried respiration. She was admitted to hospital late in the evening. Next morning, when seen at visiting hour, she was unconscious. The portio dura nerve on the side of the diseased ear had become completely paralysed, having been free from any loss of power on the previous day. The patient was evidently dying of intrathoracic inflammation, but her condition was such that an exact examination could hardly be made. A remarkable condition of the eyes existed; both conjunctivæ were distended with yellow serum, which hung as a tear between the lids.

On examination after death, which occurred shortly after visiting hour, the lungs were found to be occupied by numerous patches of inflammation, passing in some parts into the condition of secondary abcesses; both pleural cavities were inflamed. The appearances in the chest showed clearly that death had been caused by purulent poisoning. On

opening the cranium the brain was found congested, but free from abscess or any sign of arachnitis. On raising the dura mater off the petrous bone, purulent matter was seen to escape from the petrosal sinus at its anterior extremity. The cavernous and circular sinuses were filled with similar matter. The cavity of the tympanum was occupied by an abcess which had produced absorption of the bone, both of its roof and floor. The portio dura nerve was found lying across the roof, but its bony canal was completely absorbed. The dropsy of the conjunctivæ had apparently resulted from the obstruction of the cavernous sinus, at least none of the conditions which usually give rise to this condition existed, such as albuminuria, &c.—*December 3rd*, 1870.

Tumour of the Brain—Congestion Papilla.—Mr. Swanzy said the case which he was about to bring under the notice of the Society was of interest to the physician as well as to the oculist. The patient was a private in the Scots Fusilier Guards. On the 18th of April last, having fallen out faint on the march, he was admitted to hospital in Beggar's Bush Barracks. On admission he was in a heavy, listless state; complained of headache, giddiness, and much vomiting; his pupils were widely dilated. From this time until the 18th of August, when the patient died, the general symptoms consisted in constant headache, frequent vomiting, great drowsiness, and loss of memory, so that he often forgot having had his meals. If asked a question it would be some moments before he would reply, and then his articulation was very slow but distinct. The bowels were very torpid, making the use of purgatives frequently necessary. During his illness of four months these symptoms were sometimes more and sometimes less aggravated. At one time he so far recovered as to be able to take a short walk in the barrack yard; but on the next day he was obliged to return to bed. About a month or six weeks after his admission to hospital Dr. Robinson asked him (Mr. Swanzy) to see the patient. He found the pupils widely dilated, and in the left eye all power of perception of light was lost. In the right eye there was still a slight power of perception of light. The ophthalmoscope revealed that condition of the optic disc described by von Graefe as congestion papilla. This led him to diagnose an increased intracranial pressure, dependent, probably, upon the presence of a tumour within the cranium. The ophthalmoscopic appearances were very similar to those represented in Fig. 2, Table XI., of Liebreich's *Atlas*,[a] and were beautifully depicted by Dr. Fitzgerald in the drawings taken during life. There was a great tumefaction of the optic disc. The tumefaction was very prominent, but did not extend far into the surrounding retina. The retinal veins were very much engorged, and very tortuous in their course. They disappeared

[a] Liebreich's Atlas of Ophthalmoscopy. Translated by H. R. Swanzy. London: 1870.



Fitzgerald ad nat. pinx.

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Mr SWANZY — CONGESTION PAPILLA.

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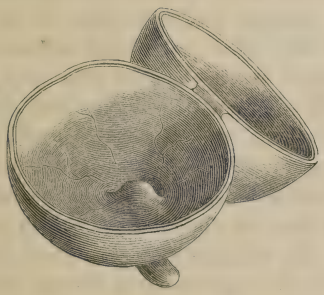
from view in some places at the edge of the tumefaction, where they were obliged, in order to reach the centre of the disc, to turn round the edge of the tumid part. This condition was produced by an increased intracranial pressure, the cavernous sinus was pressed upon, the return of blood through the ophthalmic vein was impeded, and the retinal veins became enormously congested. It was, then, not difficult to understand how, under these circumstances, a kind of venous strangulation occurred at the narrow and rigid optic entrance, through which the retinal veins have to pass out. A serous exudation occurred here, associated with the formation of cellular tissue, and hence the tumefaction. The autopsy, which was conducted by Mr. Baker, fully confirmed the diagnosis. A tumour, as large as a moderate-sized apple, was found situated in the centre of the brain. It involved the convolution at either side of the longitudinal fissure in the middle and exterior lobes, extending laterally somewhat into each hemisphere, and reaching downwards as far as the corpus callosum, but not implicating it. The tumour was a sarcoma, with small round cells, and in some places it had undergone fatty degeneration. Through the kindness of Mr. Baker, Mr. Swanzy also obtained the right eye, and it formed a beautiful preparation, in which the swelling of the optic disc could still be well seen. The patho-



logical changes in congestion papilla do not extend up the optic nerve, but cease at the lamina cribrosa, thus differing essentially from the disease known as neuritis descendens, in which the morbid changes extend from the base of the skull along the nerve to the papilla.—*December 10th*, 1870.

Tumour of the Humerus.—Dr. Tyrrell exhibited the cast preparation of a remarkable tumour, the bones of the amputation, and two drawings showing the appearances before amputation and after section of the tumour. The tumour was removed from a man who came to the Mater Misericordiæ Hospital last August. He was a groom, twenty-two years of age, and of intemperate habits. While riding a restive horse, two

years before, he was thrown, and gradually a tumour appeared in the lower portion of the humerus unattended with pain; and he continued his occupation—not riding horses, but working in the stables—until two months before his admission to hospital. About that time he consulted a neighbouring practitioner, who passed a trocar into the tumour, when some blood and serum were discharged, and a fungus remained. The tumour occupied the lower three-fourths of the humerus, and exhibited, as they saw by the cast, an enormous mass; the hand below was extremely atrophied, and ramifying over the tumour were large veins, very much resembling those seen in the breast in cases of encephaloid cancer. It was determined to remove the arm at the shoulder-joint, although he had not made up his mind whether it was of a malignant nature or not. He thought it might possibly be a soft cancer, although there were no glandular swellings; and there was no cachexia. He removed the arm, accordingly, at the shoulder-joint. The patient lost little blood, recovered without any unfavourable symptoms, and left the hospital a short time ago, having increased two stone in weight. On slitting up the tumour a remarkable appearance presented itself. At the centre of the tumour there were immense clots of blood, particularly at the lower part of it. Dr. Purser had been kind enough to make a microscopic examination of the tumour, and he stated it to be a strictly cystic sarcomatous tumour, and that these large ecchymosed patches were the result, probably, of an accidental rupture. Having macerated the bone he found that the lower part was very much expanded, and formed a thin shell. It was probable the disease commenced in the interior of the bone, and that at length a small fungus was formed. The bones of the forearm were not affected by disease. Since August he had not heard from the patient; and although Dr. Purser said the case was not one of a malignant nature, yet it was one which might recur. He thought the case worth bringing forward from the size of the tumour, the rapidity of its growth, its painlessness, and the success that attended its removal.—*December 10th*, 1870.

Pelvic Cellulitis.—Dr. Hayden said the morbid specimen he now exhibited had been removed from the body of a female, thirty years of age, who was admitted to the Mater Misericordiæ Hospital on the 4th of October, three weeks after her first confinement. At the time of her admission she presented rather a condition of general malaise than distinct symptoms that would indicate any particular organ as the seat of disease. She was greatly emaciated, but there were no hectic symptoms; her pulse was quiet, her skin cool; tongue clean and moist, and she digested food tolerably well. She also slept very well, but there was marked hyperæsthesia of the entire surface, especially of the abdomen. The slightest touch caused her to complain; but when questioned she admitted

that was rather from the apprehension than the feeling of pain. The bowels were persistently constipated; and it was found, on endeavouring to administer an enema, that the tube could not be made to pass. He made the attempt himself, and was satisfied that the instrument could not be made to pass beyond seven to eight inches; nevertheless, aperient medicines, given by the mouth, acted freely. A dose of turpentine and castor oil acted briskly, and relieved her of volumes of flatus, which had previously distended the bowels. There had been partial laceration of the perinæum, and this was the only external indication of anything wrong. In the course of a month the abdomen began to swell in a marked degree, and he then perceived that the umbilicus projected. After a short time the umbilicus gave way and matter was discharged in large quantities. This continued to flow for a month in such quantity as to flood the bed of the patient. She went down gradually, and died in the course of last week. An examination of the abdomen was made by one of the residents, but he did not carry out his (Dr. Hayden's) instructions, to ascertain, if possible, the original source from which the matter proceeded. He considered the case to be one of pelvic cellulitis and abscess, which subsequently found entrance into the cavity of the peritoneum, and made its exit at the umbilicus.

A careful examination of the uterus showed that there was no disease of that organ itself. The fundus showed that for a long time it had been bathed in thick matter, there being a deposit of pus on the surface, which was slightly discoloured.

There was a great quantity of pus in the abdominal cavity, which, as reported, seemed to proceed from the right iliac fossa. The abdominal wall in the neigbourhood of the umbilicus presented an appearance similar to that of the fundus uteri, and the aperture at the umbilicus was sufficiently large to admit the barrel of a goose quill, and it showed that the matter had been in the cavity of the peritoneum before it found exit at the umbilicus. He thought it might have travelled in the course of the urachus; and this would explain the absence of symptoms of peritonitis, which she did not evince. It will be seen, however, that the matter proceeded from the peritoneum direct, which, immediately bounding the opening, was covered with a film of fibrine and a layer of pus.

The case was of interest—1st, as to the difficulty of diagnosis; and 2nd, as regards the obstruction to an instrument introduced by the rectum, whereas medicines acted normally when administered by the mouth; and third, with regard to the actual existence of peritonitis by irruption of pus into the peritoneum without any of its symptoms. The obstruction to the tube of the enema machine may be readily accounted for on the assumption of a large abscess pressing from outside upon the bowel; whilst purgatives, given by the mouth, might, at the same time, have acted in the ordinary way.—*December 10th*, 1870.

Aneurism of the Aorta.—Surgeon I. Smith Chartres, of the 8th Hussars, detailed the following case of sacculated false aneurism of the thoracic aorta, below the termination of the arch:—

Private G. Parting, 8th Hussars, aged thirty-six years, of nervous, lymphatic temperament, by previous occupation a glass-blower, sixteen years' service, of which nearly seven were passed in India, where he had suffered from repeated attacks of intermittent fever, and one of prolonged primary syphilis (followed by secondary symptoms), treated with mercury; occasionally intemperate; without any marked indications of a gouty or rheumatic diathesis; was admitted into the Regimental Hospital, Arbour Hill, Dublin, on the 15th August, and died on the evening of the 2nd December following, 1870.

Two years previously, whilst exercising a young horse in the Riding School, the animal reared and fell over, with the rider underneath, the fall shaking the man a good deal; but, so far as could be learned, his chest was not severely, if at all, compressed; and after a detention of two days only in hospital, he apparently quite recovered, resuming his ordinary duty.

In April, 1869, he for the first time came under my observation, when he was admitted into hospital for an attack of double pleuritis, mild, circumscribed in extent, and affecting the anterior lower, or inframammary portions of the membrane. The seizure soon yielded to ordinary treatment, and in about a fortnight's time he seemed well, and was able to return to duty.

After this attack, he on several occasions complained of sharp, shooting, somewhat lancinating pains, darting in and around his chest on both sides, but more especially the left, severe, somewhat paroxysmal in character, "catching" his breath, causing considerable suffering, and attended by great mental depression, even to the shedding of tears, and a sense of impending serious illness, for the relief of which he requested decided remedies, "a blister, or anything to remove the pains," which at that time were considered anomalous, but due to intercostal neuralgia, muscular rheumatism, or pleurodynia; and the "dregs," perhaps, of his previous pleurisy (there being, however, no symptoms present of any active disease of this description). On each occurrence he experienced effectual relief from sinapisms, hot fomentations, and a stimulating aromatic cordial mixture.

In July, 1869, the regiment leaving Edinburgh, arrived in Ireland, at Dundalk, when the man was sent on detachment duty to Belturbet, when I lost sight of him for a season; nor did he come under my notice again until my return from leave of absence on the 19th of August, 1870, when I found him a patient in the hospital, into which he had been admitted four days previously under the head of asthma.

At this time he suffered from *ordinary* cough, with free mucous frothy

expectoration, "stitches" darting through both sides of his chest, but especially severe in left inframammary region, "catching" his breath on full respiration, and indeed of a similar description to those previously experienced, except that they were not now attended by undue mental depression, or quite as much suffering as before; he was also the subject of marked dyspnœa, with occasional attacks of exacerbations, resembling in some measure true spasmodic asthma, inasmuch as the paroxysms frequently came on during the night, lasted several hours, were invariably aggravated by atmospheric conditions, viz., an easterly wind, or increase of cold, or moisture in the air, and found no marked indication or relief in change of position out of the sitting posture, whilst they were not accompanied by stridor or other laryngeal symptoms, and were always relieved by sedatives and antispasmodics (lobelia and chloroform in particular). He was anæmic, delicate-looking, being evidently out of health, and bore very much the general condition and appearance of one labouring under pulmonary consumption.

Sonorous rhonchi, and moist crepitating sounds were largely distributed throughout both sides of the chest, almost altogether masking healthy vesicular respiration, which could be heard on right side, posteriorly below the scapula, faintly indeed, but in a more audible degree than elsewhere throughout; dulness on percussion behind and below, over the base of both lungs, extended upwards for about three inches from where they lay over the diaphragm; no friction sounds were now present. The heart's action was normal in rythm, but the systole was weaker, and not as sufficiently well pronounced as it should have been.

He did not improve; on the contrary, the "stitches" recurred with increased severity from time to time, confined to the inframammary regions, and not extending upwards along the neck and head, or laterally to the superior extremities; no other species of thoracic pain, either anteriorly or posteriorly, was complained of. The dyspnœa continued to be persistent, with exacerbations, as before, and *co-existent, in point of time, with those of the lancinating "stitches"* and intercostal pains. Cough, with muco-purulent expectoration, did not diminish, and he became more and more like a phthisical patient. At no period was there any hemiplegia; but his hopefulness all through that he would "battle through it" was very remarkable up to the sad end.

Early in October (about the 4th), when making my morning visit, I learned that he had passed a very bad night from incessant and troublesome cough. On entering the ward its peculiar character at once arrested my attention; it was now paroxysmal, dry, loud, with a metallic croupy ring, and immediately I suspected the presence of an aneurismal or other tumour within the cavity of the thorax, pressing upon the pneumogastric nerves; the indications of such were, however, anything but satisfactory or conclusive as to its existence, for inspection

of the chest gave no assistance, there being no throbbing, pulsation, or tumour visible; percussion afforded no certain sounds of an excess of dulness in any one spot, the base of each lung, as before mentioned, being alone excepted. The stethoscope did not reveal any peculiar or unequivocal criterion, so as to distinguish the aneurismal from the cardiac sounds; there was no murmur, single or double, or distinguishable impulse; no second centre of pulsation within the thorax could be detected. Change of posture threw no new light upon the state of matters; no fremitus was perceptible, and close compression together of the anterior and posterior walls of the chest at the termination of an expiration, gave no abnormal point of impulse or signal to the hands, already on the alert to receive such.

Of the rational signs, four of those depending upon the effects of pressure were the only ones indicative of the existence of an intra-thoracic tumour or aneurismal sac, and were—1st, The *peculiar* cough, intermittent, only occasionally present, superadded to the ordinary bronchial cough, not relieved or aggravated by changes of position or posture; unattended by stridor, hoarseness, or aphonia, paroxysmal, of the remarkable character before mentioned, which, when once heard, is seldom forgotten, and not resulting in any expectoration. 2nd, The *peculiar* dyspnœa, resembling attacks of true spasmodic asthma in this case. 3rd, The *peculiar* pains or "stitches," darting through the anterior, inferior, or inframammary regions, and the existence, in point of time, of their exacerbations with those of the peculiar dyspnœa, as previously remarked. And 4th, The peculiar fixity of the thorax, which, as though paralysed, lay motionless and at rest; whilst the diaphragm acted most energetically during respiration, which latterly was almost altogether of the abdominal character. This was a very remarkable symptom, and attracted daily attention during the latter end of the disease.

Of the remaining rational signs depending upon pressure effects none were present. There was no dysphagia; no alteration or difference in the carotid or radial pulse; no œdema; no venous turgescence on one side or both; no fixed pain in the back, invariably referred to the same spot, and not influenced by external pressure, and of a boring, burning, or corroding description; no laryngeal stridor, hoarseness, or aphonia; no tumour pressing, or throbbing visible externally; and no abnormal contraction, dilatation, or want of harmonious correspondence between the pupils.

The attacks of dyspnœa became gradually more and more frequent, causing great distress from breathlessness and almost impending suffocation, culminating in constant orthopnœa; copious muco-purulent effusion took place from time to time into the bronchial tubes, the expectoration of which, aided by the act of vomiting which the exhibition of a little boiled milk invariably produced, afforded some temporary relief; his

nights became periods of sleeplessness and much suffering, and he finally died a painful death, sinking eventually from exhaustion on the evening of the 2nd December, 1870.

Sectio-cadaveris made on the following day, fifteen hours after death.

1. *State of Thorax.*—Heart apparently quite healthy. Aorta, from heart to diaphragm, affected with atheromatous degeneration; coats thickened and roughened, presenting the usual patches, irregular, ovoid, and circular in shape, where the fine, opaque, white striæ or streaks have become aggregated together into large white opaque spots, replacing the yellow colour and elastic quality of the middle coat, where they prevail. This stage of the degeneration is best seen on the lower portion of the artery, a somewhat more advanced one being readily detected in the arch, where the connexion between the internal and middle coats along the edges of the patches have become intimate, and not easily separable, within the circumference of which the cheesy or pellaceous disintegrating process had evidently commenced, without, however, having advanced so far as to have become of a liquid form or like pus. These deposits have occurred upon the outer surface of the inner, the inner surface of the middle, and between the surfaces of these coats of the thoracic aorta.

Just below the termination of the descending division of the arch of aorta a large, sacculated, false aneurism, globular in shape, about the size of a large orange, and closely adherent to the spine posteriorly, occupying a space corresponding to, and inclining towards, the left side of the bodies of the 3rd, 4th, 5th, and 6th dorsal vertebræ, and from which the edges of the sac were with some difficulty dissected off the bones forming the back wall of the cavity, was discovered.

It was partially filled in front and to the right side, with thick layers of firm laminated fibrine, which gave to the sac in these directions a dense, hard, solid, resisting feel; the most external laminæ were the strongest and most firm, with the least amount of colour, the internal one being softer, thinner, and of a blood-red colour; its inner surface reticulated in a remarkable and somewhat venated manner. The sac to the left of the vertebræ was thinner and weaker, containing soft coagula, and was apparently increasing in size; here it would most probably have given way had the patient survived sufficiently long; a few spots of atheroma were distributed in small patches on the interior of the left portion of the sac, more especially where some layers of the middle coat were prolonged and assisted in forming this portion of its wall hereabouts, but in a sparse and scattered manner.

The opening of communication between the artery and the sac was large, free, and oval in shape, its long axis running in a vertical direction, and between the laminæ of fibrine in front, and bodies of the vertebræ behind ran a free channel for the blood, grooving the left side

of the latter somewhat, and communicating directly with the unaffected portion of the tube of the vessel both above and below the laminæ, serving thereby to keep up an unbroken, continuous passage for the current of blood. At the margins of the three openings into the sac the structure of the walls felt thicker and stronger, where portions of the internal and middle coats had given way and become retracted, leaving the company of the external sac, which beyond formed the chief constituent of the aneurismal sac, and which was seen coarse and rough on its internal surface.

The lower half of the anterior and left surface of the body of the third, and the anterior and left surface of the bodies of the fourth, fifth, and sixth dorsal vertebræ, were much eroded and worn away. The four intervening intervertebral substances having resisted the destructive agency, were spared, and found projecting prominently forwards, in marked contrast with the eaten or worn down cancellous osseous tissue. The tumour, about ten inches in circumference when *in situ*, projected in front into the mediastinal space, and appeared more prominently forwards than in any other direction; and although extending laterally to the left side of the bodies of the vertebræ somewhat, yet continued to keep an almost central position in front of the spinal column.

It evidently, in the first instance, sprang from the posterior and right lateral wall of the artery, the dilatation of the left side being clearly of a much more recent date, where the sac was thinner, weaker, and but thinly coated by one layer of soft red fibrin, and where the tendency to increase in size was obvious, so that it is probable it would have eventually protruded externally and manifested itself outwardly to the left side of the spinal column at the inferior angle of the left scapula, had the patient lived long enough; that its increase was not more rapid, taking into consideration the attenuation of its sac in this direction, is due to the fact of the direct momentum of the blood-current being evidently directed rather *backwards* against the spine, owing to protrusion forward by the tumour of the upper opening of the aorta into it, which lay on a plane considerably anterior to that of the lower opening of this vessel into the sac of the aneurism—a condition, too, which is further evidenced by the grooved channel in the left of the bodies of the vertebræ, formed, no doubt, by the continued friction of the stream of the flowing blood.

The lungs were both extensively affected by chronic inflammation of the bronchial lining membrane, which was softened, thickened, velvety-looking, and of a dark purple colour. The tubes presented dilatations of calibre in many places, more especially the smaller ones at the base of both lungs, where they were surrounded by and contained in consolidated pulmonary tissue, apparently the remains of unresolved inflammatory action. There were, however, no indentations or pouches found in their walls.

Both lungs were freely saturated with muco-purulent, bronchial secretion, which flowed out copiously when their substance was cut into. There were no tubercles discernible anywhere in the pulmonary tissue in any stage or form. Old pleuritic adhesions were found on both sides, below and in front, corresponding with the inframammary regions of the thorax.

2. *State of Abdomen.*—The liver was enlarged, tough, and heavy, in the first stage of cirrhosis. The spleen was small, shrunken, tough, fibrous-looking, and of a dull, white, opaque colour externally. The other organs examined appeared to have been healthy.

Remarks upon the above Case.—From the history it would appear probable that a state of general constitutional cachexy was established by four causes—1st, Prolonged residence and military service in a tropical climate, extending over a period of seven years nearly; 2nd, Repeated attacks of intermittent fever whilst there; 3rd, Continued poisons of syphilis and mercury; 4th, Occasional acts of intemperance, inducing the atheromatous degeneration of the aorta, and thus predisposing the vessel to the aneurismal disease.

The shrunken, tough, and inelastic degenerated condition of the spleen, when taken in connexion with the attacks of intermittent fever above mentioned, and the atheromatous conditiun of the aorta, is well worthy of notice, if it be true that this organ is concerned in elaborating the albuminous formative materials of food, and storing them up, to be gradually introduced into the blood, according to the demands of the general system; the small amount of fatty matter in such plasma, leading to the inference that the gland has little to do in regard to the preparation of material for the restorative process ("Keiller's Physiology"); as, if the spleen be injured in function or structure, it is but reasonable to suppose that a prime source of formative albuminous material being, as it were, cut off, the non-azotized constituents of structure could be substituted instead, and becoming predominant and deposited in excess of the former, may in this way have caused the fatty degenerated state of the aorta, to which fibrous tissue, and more especially the coats of arteries, are said to be peculiarly liable. This is, of course, a suggestion merely thrown out, but is, no doubt, worthy of some consideration and further elaboration in detail.

The point of election of the tumour, or its selection of situation below the termination of the arch, also deserves remark, as thoracic aneurisms are formed, in order of frequency—1st, from the ascending portion of the arch; 2nd, from its transverse portion; 3rd, from its descending division; and lastly, from the descending aorta below the termination of its arch, an experience supposed to be explicable, by the fact that the commencing portion of the aorta is that in which atheromatous and calcareous degeneration of its coats most frequently takes place, and on which the impulse

and strain of the blood-current is necessarily most felt (Dr. Fuller). Out of 703 cases of thoracic aneurism collected by Dr. Sibson, seventy-one only were found to spring from the descending thoracic aorta below the arch, or as nearly as possible about one in every ten.

It is, perhaps, then, just to assume that in this case there was a patch of atheromatous deposit, formed in the exact locality of the aneurism, either first, in point of time, or which degenerated more rapidly than any of the others elsewhere, and thus spoiled the integrity of the artery in this particular situation, rendering it powerless to resist the force of the current of the blood, which latter, however, may be considered, in a much less degree, an exciting cause than the defenceless condition of the arterial coats, the inner and middle ones having been doubtless, to a great degree, displaced by the soft and pultaceous disorganization which disintegrated them. It would also seem probable that this early-formed, or most rapidly mature patch, was a large one, and originally of an oblong or ovoid form, judging from the size and shape of the opening of communication between the artery and the cavity of the sac, as seen in this particular specimen.

The immediate cause of its production may or may not have been the fall underneath his horse in the riding school, received about two years ago, there being no distinct evidence to show that his chest had been squeezed or crushed; and the succeeding attack of double pleuritis verified by the *post-mortem* appearances, served to obscure and mystify the exact nature of the early lancinating pains, which were considered to belong to pleurodynia, more especially as they were accompanied by so much mental depression; they were doubtless the symptoms of aneurism; *first in order* to them succeeded in rotation—*ordinary bronchitis* with dyspnœa, the latter afterwards becoming strangely exacerbated by paroxysms; then early in October the *peculiar cough*, which first aroused suspicion of the exact nature of the disease; and lastly, *the peculiar abdominal breathing*, with a motionless condition of the chest, and this is the relationship which the phenomena appear to have stood in regard to each other.

The pressure of the aneurismal tumour upon and the consequent irritation of the bronchus, and the plexiform anastamoses of the pneumogastric, sympathetic, accessory, cervical and dorsal nerves, regulated the occurrence of the symptoms due to the effects of this pressure which were present; but it is not by any means as easy a task to account for the remarkable absence of many of the usual signs, which formed so interesting a feature in this particular case, the difficulties of which were not lessened by the fact that the paroxysms of dyspnœa resembled very closely those of spasmodic asthma, whilst the morbid state of the bronchial tubes and lining bronchial membrane assisted much in mystifying the true nature of the affection.

But as to the *negative signs* so conspicuous by their absence, they only serve to show how imperfect are our means for the diagnosis of thoracic aneurisms, and uncertain even when we invoke the aid of the stethoscope to assist in confirming the general symptoms. In this case percussion elicited no satisfactory result; such, indeed, could hardly be expected from the deep-seated locality of the tumour and its surroundings; its projection *forwards* into the mediastinum; and from its containing but fluid blood in the only direction where it deviated at all from its central position in the middle line in front of the bodies of the vertebræ, viz., slightly overlapping them on the left.

That there was no appreciable murmur or impulse, single or double, can only be explained by the fact, that between the laminæ in front and the bodies of the vertebræ behind ran a free channel for the blood, grooving the left side of the latter somewhat, and communicating directly with the unaffected portions of the tube of the vessel, both above and below the tumour, serving thereby to keep up an unbroken continuous passage for the current of blood through it; and besides, as Dr. Stokes has observed, there are no peculiar or constant phenomena belonging to aneurism by which the signs produced in the tumour can be distinguished from those of the heart; whilst an extremely weak, almost imperceptible, impulse may attend even a large aneurism of the aorta.

There was no dysphagia, probably because the œsophagus, composed of a yielding and accommodating structure, was not pressed by the tumour against any very resisting medium; it may have been pushed over to the left by the projecting right solid portion of the aneurism, and thus rested upon the sac, which only contained fluid blood in this particular direction.

The tumour, situated below the origin of the great vessels springing from the arch could not in any way have influenced either the carotid or radial pulse, and it exercised no sufficient amount of compression upon the venous trunks to produce œdema or venous turgescence of one side or both, or upon the recurrent nerve, to excite or establish laryngeal stridor, hoarseness, or aphonia.

That there was no fixed pain in the back, invariably referred to the same spot, and not influenced by external pressure, appears a somewhat remarkable circumstance, considering the amount of erosion of substance the bodies of the dorsal vertebræ had undergone, and is one which admits of no more satisfactory explanation than the fact of its being known sometimes to have existed to a considerable degree, even when the bones were found, after death, quite uninjured.

The aneurismal tumour was evidently situated too low down to have exerted any pressure upon the cervical sympathetic, its ganglia, or connexion with the spinal cord, and accordingly "no abnormal contraction,

dilatation of, or want of harmonious correspondence between, the pupils of the eyes was observed during life."

There appear to be no rational grounds for assuming that any special conditions peculiar to and inseparable from his particular duties or mode of life as a cavalry soldier existed, so as to render him exceptionally liable to the production of an aneurismal tumour, *after the atheromatous degeneration of the coats of the aorta had once become fully established*, when any accidental occurrence would have proved sufficient to have originated, and afterwards fully developed, the disease under which the man laboured, in any individual under the ordinary circumstances of any other employment or avocation whatever, in an analogous station or position in life.

That the disease was supposed to have been an aneurismal rather than any other known description of intrathoracic tumour, in the absence of true stethoscopic criteria, of course rested very much upon conjectural and speculative grounds.—*December* 10*th*, 1870.

Disease of the Prostate Gland, &c.—Dr. Tyrrell said the morbid specimens he exhibited were removed from the body of a man who died in the Mater Misericordiæ Hospital on last Wednesday. The patient was admitted on Monday—was, in fact, carried into the hospital, labouring under retention of urine; and when he saw him immediately after he presented a wretched appearance. He was unable to stand without assistance; his face was pale; he was partly wandering, and complained bitterly of the state he was in. His son stated that he had been labouring under partial retention since Saturday; that he scarcely passed any water; that he did not sleep; was in great agony; and he therefore brought him in a car to the hospital. The history given of the case was an unsatisfactory one. The old man could scarcely state that he had stricture for thirty years; that he occasionally suffered from retention; was relieved; went on quietly for a period of some months, until he was exposed to cold, and then suffered from retention again. His (Dr. Tyrrell's) assistant, Mr. Curran, passed with facility a No. 1 catheter. The patient was put to bed; warm jars were applied to his feet, but towards evening he became comatose, although the bladder had been relieved. He remained in this state until Wednesday, when he died. On Tuesday morning, the day after his admission, the bladder was again distended; and on any attempt to pass a catheter the patient, although in a state of insensibility, resisted very much. He (Dr. Tyrrell) failed on that occasion to pass a catheter; and his friend, Dr. Hughes, being present, he asked him to try, and he succeeded in getting it in without difficulty; the urine which flowed out was acid. Its specific gravity was 1026. The first thing noticed on tracing the urethra backwards was that there was in front of the bulb a stricture, not very tight, and capable of admitting

a No. 3 catheter; the mucous membrane was very hard and rough, and in front of it was a false passage which extended upwards and towards the right about two inches. It must have existed for a long time for the new channel was lined with mucous membrane. The existence of the false passage explained the difficulty experienced in getting a catheter into the bladder. The bladder itself was in a state of congestion, some parts being of bright scarlet hue. The muscular coat was very much thickened, and some of the fibres were elevated so as to present a resemblance to the columnæ carneæ of the heart. On the left side, the ureter, about its centre, was dilated to the size of the small intestine; whereas the right one differed little from its normal size. In the kidneys there was not much evidence of disease.—*December 17th*, 1870.

TRANSACTIONS OF THE CORK PATHOLOGICAL AND MEDICO-CHIRURGICAL SOCIETY.[a]

Address by Croker-King, M.D., *President.*

Gentlemen,—Having, through your kindness, been selected President of the Cork Medical and Surgical Society, the duty devolves upon me of opening the Session by a few observations from the Chair. I am deeply sensible of the high compliment you have paid me by selecting me to fill the first office in your Society, and I feel your kindness the more when I consider the higher claims of others to the distinction you have conferred on me, a comparative stranger.

It is now three years since I was introduced to this Society, by one of its most distinguished members, and on every occasion, within my reach, I have availed myself of the privilege of attending your meetings.

During the last Session I was unable to be present, except on very rare occasions, in consequence of extra official occupation in connexion with an important bill before Parliament, which caused me to be frequently absent from home. During the present Session I shall endeavour to be more constant in my attendance, but I fear that the nature of my official appointment may render it impossible for me to be present as often as I would wish, so I must crave your indulgence, and ask you to attribute my absence, not to diminished interest in your proceedings, but to the cares and duties of the public service.

On looking over your published transactions and your minutes, still in manuscript, I feel myself justified in stating that the communications laid before the Society, and the ability with which the subjects were handled, would bear favourable contrast with the transactions of many societies, and are inferior to none.

[a] These Reports are furnished by the Secretary, Dr. W. H. Holmes.

As an old member of many of the medical and scientific societies of the metropolis, I have had some experience of the manner in which the discussions have been conducted by those bodies, and I can confidently affirm that on every occasion upon which I have been present at your meetings, the debate has been carried on, in many instances no doubt, with warmth and enthusiasm, but always in that good spirit and gentlemanly feeling which should be inseparable from scientific discussions; I have never heard an expression which would indicate the existence of professional rivalry, but, on the contrary, although differences of opinion have existed, and must exist so long as the healing art is not a fixed science, still the discussion has always had the one tendency, the elucidation of the subject under consideration by either new and intelligent views, or expositions, the result of long experience and observation.

The communications made to this Society hitherto have been principally connected with the absolute practice of surgery and medicine. Most interesting pathological specimens have been exhibited, and ably connected with the symptoms which had proclaimed their existence; and the correctness of the diagnosis has been amply confirmed by the *post-mortem* revelations, and no more important subjects could occupy the attention of the Society; but I would desire to see the labours of the Society more extended. I would wish to see more attention paid to diseases of tissues, considered distinct from the diseases of organs. I would desire to see the histological element of disease more frequently elucidated; for example, we have had communications on the subject of heart disease, of lung disease, of liver disease. Now these terms are indefinite, and signify merely that, in each of the cases, some of these organs were diseased, but when we come to analyse the question, we find that the disease, whatever it may have been, had not invaded the organ, but merely one of the tissues of which the organ is composed, and that our remedies should be directed, not to the arrest of disease of the organ, but of the disease of one of the tissues of which the organ was composed.

Medicines do not act on organs, they act on tissues; for example, strychnine acts on one set of nerve-fibres, woorara poison on another; mercury acts on certain tissues more actively than on others; ergot of rye on the involuntary muscular fibres through the organic nerves; and tobacco on a different nerve-structure: and so on, instances of the histological action of medicines could be easily multiplied.

A learned member has promised, perhaps, to lay before the Society a case of heart disease, but the first question to be entertained is which of the tissues of the organ has been primarily affected, and can we associate the early symptoms of the disease with the first invasion of the same tissue in, possibly, a distant part of the body. As the pathological specimen is presented to us we see, perhaps, enlarged heart,

great hypertrophy of the muscular structure, constriction of the mitral orifice, and permanent patency of the aortic opening; and what have been the symptoms which have characterized this fatal case towards its close? Vibrating, or it may be visible pulsation of the larger arteries, expectoration of blood, dyspnœa, œdema of the lungs, and of the extremities, and the morbid appearances would readily account for all those distressing symptoms; but let us trace the case to its origin, and we, perhaps, find that some years ago the patient suffered, it may have been, from a slight attack of articular rheumatism, and that, during the course of the attack, the patient suffered more or less from uneasy sensations in the region of the heart, and more or less palpitation; and this cardiac affection may have been almost unnoticed, but all this time the seeds of death have been planted, lymph has been deposited around the vascular openings, producing little disturbance of the circulation at the time, but subsequently undergoing that process of contraction to which it is liable, and thus constricting the mitral opening, and pinning down the aortic valves so as to render them useless, and causing the elasticity of the aorta and great vessels to embarrass instead of assisting the circulation; the blood unable to escape through the mitral openings is thrown back on the lungs, and the left ventricle becomes hypertrophied in order to compensate for the loss of power consequent upon the destruction of the office of the aortic valves.

But why should the rheumatic affection of the joint have been transferred to the heart? Histological medium explains the phenomenon.

What is a joint? It consists of bones lined by a smooth polished membrane—the synovial—moved by muscles, and tied together by fibrous tissue. What is the heart? Does it not consist of the same tissues, with the exception of bone, and even this we have in some of the lower animals, and as the result of disease even in the human subject, but the tissue which is peculiarly the seat of the rheumatic affection in the joint is the fibrous tissue, and it has its true analogue in the fibrous covering and the fibrous lining of the heart; and hence all wonder ceases at the frequent complications of heart disease, and external rheumatism, as it is merely the invasion of a similar tissue, in the same individual, by the same disease.

Again, in other diseases and injuries, the same material, coagulable lymph, is effused; we see it in inflammation of the liver and lungs, when the disease attacks the interlobular areolar tissue. We see it thrown out around the fibrous sheath of the urethra; around the œsophagus in scalds and burns from acids, &c.; around the orifices of the heart in endocarditis; and as a substitute for skin, when true skin has been destroyed by burns; and we see in all those cases the texture follows its natural laws, namely, slow contraction, a property with which it is endowed, and which is inseparable from its nature. Slowly, but surely, it contracts in the case of the liver compressing the liver structure and rendering its surface

irregular and hob-nailed. Contracting the lung, as we see in cirrhosis; shutting up the urethra as in stricture; contracting the cardiac orifices, and destroying the office of the valves; producing stricture of the œsophagus; and in burns, where we have an opportunity of witnessing the great power with which the structure contracts—webbing the fingers together, winging the arms to the side, pinning the chin to the sternum, or even, in some unhappy instances, drawing down the hairy scalp between the shoulders.

I allude to those familiar results to impress upon you the extreme value of generalization in observing disease and its consequences; and that in studying pathology we should bear in mind not only the peculiarities of certain structures, but make ourselves well acquainted with the physiological action which particular medicines exert on each of the tissues which enter into the structure of man.

Another subject, which is well worthy of consideration, is the intimate connexion which exists between disease and the perfection of the nervous system.

To the nervous system we owe both sensibility and voluntary motion, those twofold sources of all our relations with the external world.

It presides over all organic functions, and, while it is the origin of all the normal phenomena of life, it is also the origin of pathological action.

In proportion as we ascend the scale of animal life, we see the nervous system acquire *greater* development, and, at the same time, we observe that *diseases* become *more frequent*, more variable in their form, and more complicated in their nature. And why should this coincidence astonish us? Are not all our organs dependent directly on the nervous system, and if we take, one by one, the different systems of the animal economy, it can be shown that all the symptoms of the diseases, to which they are liable, may be produced by direct irritation of their corresponding nerves.

What, for example, are the principal signs of the affections of the respiratory organs—cough, dyspnœa, and increased bronchial secretion? are not these the symptoms which most frequently proclaim their existence?

Now all these phenomena can be produced at will by direct excitation of the pneumo-gastric or certain other nerves. We can even call into existence the anatomical lesions incidental to pleurisy and pericarditis.

By exciting the solar plexus and its efficient branches we can determine both diarrhœa and dysentery, together with the anatomical lesions which habitually accompany them—even peritonitis can be thus established; and we can produce directly pleuritis with purulent deposit in an animal previously enfeebled by the simple division of the great sympathetic nerve.

Deprive a muscle of its nervous supply, and you will have, as a consequence, fatty degeneration.

Deprive a bone of its nervous supply by ligature of the nerves which enter into the nutritious foramina, and the result will be increase in size of the cells of the lamellar structure, increased vascularity, and all the phenomena of rickets following in rapid succession.

If you divide the nerves going into the kidneys, during the first few days which follow the operation, albuminuria is produced; shortly afterwards inflammation of the kidneys sets up; they then mortify and become decomposed, and finally act on the economy like a septic poison, and the animal dies.

I have already observed that, amongst animals, the higher the development of the nervous system the more frequent and complicated are the diseases to which they are liable; and this statement even admits of more particular application, for, even amongst the same species, those individuals whose nervous system is most highly cultivated, frequently succumb to disease which the less sensitive creature would successfully resist. Amongst the human species examples of the truth of this statement present themselves daily to the observant physician; and it is a fact well known to experimenters on living animals that highly-bred creatures, such as thorough-bred horses, greyhounds, or pointers, do not answer for experimental operations, as they are so highly sensitive that fever is induced by the wounds inflicted by the operator, and they frequently die.

To investigate, in a strictly scientific manner, the origin and progress of disease, great labour is necessary, and uncommon powers of observation are required; a knowledge of vitality and its eccentric laws must be attained, and thus difficulties almost insurmountable must be encountered. Step by step the eminence must be attained; there must be no faltering, no despondency, no turning back. Let the watchword be "onward," and the result will be victory.

And as Nature's sun proclaims his approach by blazing scarlet clouds running before his chariot, and heralding his majestic approach, and then rising upon the world brightens and awakens nature, so may the orb of medical science rise upon the earth, and by its effulgence dissipate the darkness of superstition and empiricism.

If the early inquirers in the physical sciences were deterred by apparently insuperable obstacles, I ask you would the magnificent results which followed their inquiries have been obtained?

Does it not appear incredible that man, situate upon our planet 237,000 miles from the moon and 95,000,000 of miles from the sun, can actually weigh these worlds against each other, and determine their relative masses of matter, and even their chemical composition.

Vision, the most noble and exalted of all man's senses, failed him in his efforts to penetrate into space. Distant luminous spots in the heavens

by their brilliancy revealed their existence, but they were separated from him by a barrier of distance apparently impassable.

It has been well observed that, if gifted with powers of ubiquity, man could sweep out from earth towards the distant planets to gain a nearer and more accurate view, or else if he could bring them down from their lofty spheres to subject them to his scrutinizing gaze, how much he then could learn. But how hopeless would seem the accomplishment of either of these alternatives; but who shall prescribe the limits of human genius? By the potent aid of the telescope the astronomer is no longer bound hopelessly to his native earth, but the eye, now gifted with superhuman power, ranges the illimitable fields of space.

But the progress of human intellect and invention is not confined to observations of the heavenly bodies. Man's invention has all but obliterated space. The rapidity of intercourse has practically approximated nations; and man selecting the lightning as his messenger, our ideas are transported to distant climes with the rapidity of thought; and, enlisting the sun as our artist, in an instant we produce portraits which for accuracy of detail and minute precision stand unrivalled in the world of art.

Such results having been obtained, may we not look forward to great results in medicine. Were we to think that medical science was exhausted, that every fact had been positively ascertained, how many ambitious minds would turn disheartened from a pursuit in which there lay no path of glory.

But I am well assured that a glorious field for the exercise of intellect is still uncultivated, that great problems of life are still to be interpreted, and are fast being unravelled. It has been beautifully expressed by a distinguished writer—"The stream of knowledge which, not many centuries ago shallow and narrow, toiled to wear away or avoid the obstacles that impeded its course, *now*, fed by a thousand new sources, flows along deep and rapid, sweeping away every obstacle, and defying all human oppositon.

"Mankind thus approaching a maturity of collective intellect, we are every day surprised at the results obtained by the vast mass of mind now diligently employed in the acquisition of knowledge. What was formerly the *termination* of success is now but the *commencement*—what was formerly deemed *unattainable* is now *elementary;* so that it is impossible to foresee how far improvements may be carried.

"A short and transitory existence has been allotted to our bodies—individuals die and generations pass away; but the common intellect of mankind fears not the same fate, nor shows so *brief* a mortality.

"The reason of man is now more fully employed than at any former period; a vast store of mental power, a vast mass of mind, is everywhere

at work: what formerly was vainly attempted by the labour of a few is now easily accomplished by the exertions of the many.

"The empire of reason—extending from the Old to the New World, from Europe to our Antipodes—has encircled the earth, the sun never sets on her dominions.

"Individuals must rest, but the collective intelligence of the species never sleeps. At the moment one nation, wearied by the toils of day, welcomes the shades of night and lies down to seek repose, another, refreshed, rises to hail the light of morning—and thus speeds the noble work of science."

Great progress has of late been made in many sciences connected with the inorganic and also in the living world; but much has still to be learned in connexion with vitality, with life, that wonderful, inexplicable state to which all things are subordinate, and which impresses on matter conditions to which there is no parallel in inorganic nature.

We can imagine our first parent to have been perfect in every particular of construction: the machinery of life, the anatomical tracings, the chiselling of the features, all bearing the impress of a Divine Artist, and Adam thus to have been the very perfection of manly dignity and beauty; but still all is calm and motionless as the grave. It required the Divine inspiration to set in motion the inanimate form—God breathed into Adam the breath of life: man became a living soul.

Cotemporaneously with the introduction of the vitalizing agency the several functions came into play, and the lifeless form, which hitherto presented only the ordinary physical properties of matter, became the seat of new and wonderful operations to which nothing even analagous is to be found in all the combinations of inert matter. Endowed with reason, man alone seems capable of understanding (to a certain degree) the construction of his own body, the uses and functions of its various parts, and is the only animal that has attempted the problem of explaining himself. Man alone has considered the causes of disease, and sought to avert its fatal consequences by the suggestions of reflection and experience.

Wonderfully and fearfully made, our bodies, composed of matter in itself inanimate, are governed during health by the laws of vitality, and a constant struggle between life and death is of momentary occurrence in the penetralia of the system. Inorganic bodies owe to external causes their decomposition and destruction, but living bodies perish from within. The moment we preserve organic matter from destruction we render it incapable of life; the moment we begin to breathe that moment we begin to die, so completely are the germs of life intermixed with the seeds of death; and, should the being escape accident or disease, nevertheless it is doomed to perish by the slow and unerring operation of natural causes inseparable from its nature and coeval with its birth; and yet, so beautifully perfect is the structure of our bodies, that we are unconscious

of this mortal struggle which is constantly maintained in every member and in every organ, and thus we pass through "the age that melts in unperceived decay." But when life ceases a change commences which soon destroys the shape and texture of every part. Death, triumphant, seizes its unequal foe, and as it has been poetically expressed—"The most favoured form is buried from our view, and thus the yawning tomb receives the lifeless mass, and in a few short hours some simple element is all that remains of a form which a moment before was, perhaps, the object of the fondest affection." But the skilful physician may have it within his grasp to avert the hand of death, or it may be to call back the waning intellect or restore reason to its seat, and thus to exercise an art almost divine.—*October* 6, 1870.

Two Cases of Puerperal Convulsions successfully treated by Chloroform. By E. R. Townsend, jun., M.D., L.K.Q.C.P.I.; Physician South Infirmary and Lying-in Hospital, Cork. Read December 5th, 1870.

Case I.—A lady of a thin spare frame and sallow complexion, of a highly nervous temperament, in the seventh month of her fifth pregnancy, awoke on the morning of Wednesday, March 17th, 1869, complaining of great headache and some sickness of stomach; shortly after the pain increased, and she began to complain of a strange feeling in the head. She had got a slight blow on the head a week before, from a window which was suddenly blown in by a gust of wind. She was seen by Dr. Townsend, senior, about twelve o'clock, when she said she was suffering from one of her bilious attacks, but that her head was worse than she ever remembered; that she was giddy and queer, and that there were flashes before her eyes and noises in her ears; she partly ascribed this to the effects of the blow on her head. She was immediately ordered a calomel pill, a senna draught, and an assafœtida enema. Her memory became affected very soon, as she evidently wanted to have her husband sent for, but could not remember his name. Before she could get either the draught or enema she got a violent convulsion, which lasted some time; as the convulsion subsided she became restless, trying to throw herself out of bed, and tossing her arms about violently, but without regaining consciousness. Dr. Day saw her at one o'clock, and administered the enema, which was retained; she struggled violently while it was being given; there was no action from it for several hours. In about an hour from her first seizure she appeared to become somewhat conscious, opened her eyes, looked round, and immediately turned her head round to her left shoulder. Her eyes being turned up and fixed; her limbs became rigid, and she went off into another convulsion; her teeth were tightly clenched, the respiration very laboured, and hissing gradually becoming stertorous. When the convulsions subsided, she remained quite

comatose, with loud stertorous breathing and bloody froth on her mouth; pulse very rapid and weak. Cold water was freely dashed on her face during the fit without any apparent effect, and a piece of a towel twisted into a rope was placed between her teeth to prevent injury to the tongue, which had been bitten in the first attack. I saw her at two o'clock; about half-past two she got a third attack equally severe, and lasting several minutes. A waterproof sheet was now placed under her head, with the end hanging over a basin, and a stream of cold water was poured on her head, from a height of a couple of feet, and allowed to run into the basin off the sheet. Mustard sinapisms to the legs and chest, and stupes of hot water and mustard were constantly applied to the feet and legs; the pulse was rapid and feeble. On examination per vaginam, the os was found closed, and no signs of labour present. A few grains of calomel and James's powder were with great difficulty put on the tongue, and washed down with a little water. In less than an hour another fit came on, and again another in less than half an hour. She was now quite comatose; breathing stertorous; the tongue protruded about half an inch beyond the teeth, which were firmly clenched and driven into it, so that not even the handle of a spoon could be passed between them. The fits continuing, and the intervals between them lessening, I made another examination per vaginam, and found that the os was softer and inclined to dilate, as the opening was nearly the size of a sixpence; the edges soft, the membrane could be felt, and the head presented. As all treatment had failed to arrest the attacks, and as they were so very frequent and violent, it was evident that if the primary cause of irritation was not removed, and that soon, the case would end badly. We therefore determined to endeavour to bring on labour, and after some difficulty I succeeded in rupturing the membranes with my finger nail. Uterine contraction set in almost immediately, and at five o'clock the os was dilated to nearly the size of a florin. She appeared to wince a little when the uterus was contracting as if she felt some pain, but gave no other evidence of feeling it. At seven o'clock the fits having become almost incessant, without any marked interval between them, and the pulse being extremely rapid and very feeble, the respirations always stertorous, being at times almost entirely suspended from the violence of the spasms, it was evident that if not speedily delivered she could not live many hours. As the os was still not much larger than half-a-crown, one of Barnes's india-rubber dilators, was introduced by Dr. Harvey, to assist the dilatation. In about twenty minutes the os being now nearly half dilated, and the head low down in the cavity of the pelvis, we thought she might be delivered by the forceps. Dr. Harvey, after some difficulty, in consequence of the convulsive action of all her muscles, succeeded in applying Churchill's forceps; these were used, as they were the slightest pair of forceps we had; but the os now became so rigid

that it was impossible, without great danger of lacerating the cervix, to extract the head.

As the imminent peril of the patient would not now admit of any further delay, Dr. Harvey, Dr. Day, and I agreed that there was only one chance of saving her life, and that was to open the head, which was accordingly done; the forceps was left on, and the perforator applied between the blades; when the head was lessened the forceps was used as an extractor, and the diminished head brought through the os; the child was rapidly brought away; the placenta soon following, the uterus contracting without any hæmorrhage. From the moment of delivery the convulsions seemed to subside for a time, as there was a clear interval of one hour without one. She lay perfectly insensible, with loud stertorous breathing; the tongue protruded, and the teeth firmly driven into it. After a space of one hour she got a convulsion without any premonitory symptoms, which was very violent. Having now procured chloroform, we determined to try the effects of it, and when another fit came on, which it did in about another hour, a handkerchief wet with it was applied to her mouth, but seemed to have no effect on the fit. In a short time there was marked twitching of the eyelids, and stiffening of the fingers, the sure signs of another fit coming on. The chloroform was now freely used, and in a few seconds the muscles, which had rapidly become rigid, became again relaxed; the twitching of the eyelids subsided, and the threatened attack passed off. We hoped from this that the chloroform would now be able to control the fits, but in less than half an hour the twitching returned, and passed so rapidly into the convulsion there was not time for the chloroform to act. Again and again the premonitory twitches appeared, and if the chloroform was ready on the handkerchief, and immediately applied, the fit was generally checked; but if the convulsive action had extended to other muscles before she got under the full effects of the chloroform, the fit went on, and lasted just as long as before delivery; but by constantly watching at the bedside, one with the bottle of chloroform, and another with the handkerchief in hand, always ready to apply it at the slightest sign of muscular twitching, many fits were warded off altogether, and although, after several threatenings, a fit was sure to come on that nothing would stop, still the intervals between the attacks was gradually increased, so that frequently a space of nearly two hours would elapse without a regular fit; this continued until four a.m., when she had a very severe attack. From this out the chloroform was able to control the twitchings when freely and quickly used. She was thus kept under the influence of chloroform from nine o'clock on Wednesday evening, till late in the night of Thursday, when, as the twitchings had quite ceased, the chloroform was stopped; from first to last she had inhaled, of course including waste from evaporation, nearly sixteen ounces of chloroform. I omitted to mention that her hair had been cut close

with a pair of scissors, previous to her delivery, and a bladder of ice had been kept constantly applied to her head, instead of cold water, but without producing any apparent effect; the enema had passed off early in the evening; an attempt was made to repeat it, but the moment the tube was introduced, it brought on a convulsion, and was not again attempted. Ice was constantly applied to the tip of the swollen tongue, to keep it moist; hot stupes were constantly applied to the loins, as there had not been any secretion of urine for many hours; about an ounce had been drawn off before the introduction of the forceps, which was highly albuminous. On introducing the catheter in the morning, about half a pint of albuminous urine was drawn off; during the day the secretion increased. As the fits subsided a blister was applied to the nape of the neck, and behind the ears; these rose well, but the profound coma still continued. At nine o'clock on Friday morning, a consultation was held, at which Dr. Towsend, senior, and Dr. W. C. Towsend were present, and as the patient was very weak, the question of supporting her with injections of beef-tea was discussed. She had had one administered with a little brandy in it during the night, which was retained; but the danger of reproducing the fits by exciting reflex action in the disturbed state of her nervous system, was thought to be so great that we feared to repeat the attempt. About noon, we succeeded in inserting a boxwood screw between the teeth, where there was a jaw-tooth deficient, and by gradually tightening the screw, the jaws were separated sufficiently to allow the point of a spoon to be passed between the teeth, and a few spoonfuls of water were got into the mouth; these were swallowed with some difficulty, but by persevering, the power of deglutition gradually increased, and as soon as she could swallow, without danger of choking, chicken broth was substituted for the water, a teaspoonful being given at a time. By this means she got down some nourishment. Towards evening she could swallow freely, and gradually was able to open the mouth, and drink from the spoon. She became by degrees more conscious, and on Saturday morning, was able to look about her; but her tongue was so swollen and sore, she could not speak intelligibly. She continued to improve, and eventually recovered perfectly, but had no recollection of her illness, and for several days could not believe she had been confined; the urine continued albuminous for nearly a week, when it quite subsided. Her head was very confused, and she was subject to a very severe pain in the back of her head for several months after.

The tongue gave a great deal of trouble, as it was so swollen and lacerated from the long-continued pressure on it, that it was several weeks before she perfectly recovered from it.

The second case was that of a Mrs. Looney, aged twenty, who was admitted into the Lying-in Hospital on Sunday, June 20th, 1870, in labour of her first child. She was very poor and badly fed, she

and her husband having had only six shillings a week to live on during the winter months. They lived in an unhealthy lane in the city. Her eyes were somewhat puffy, and she complained of headache during her labour. I saw her about two p.m., and found that her labour was slow, the os nearly fully dilated, and the head in the cavity of the pelvis. There appeared to be plenty of room in the pelvis. The pains were strong; she continued to complain of her head. About four o'clock, without any warning, she suddenly became convulsed; the fit was very severe, and long-continued. Her face was frightfully distorted, the eyes turned upwards, leaving only the whites visible; her arms and legs thrown about, and the whole body violently agitated. After the fit passed off, she remained perfectly unconscious, lying in a state of profound coma. In half an hour she got a second fit. Chloroform was now administered, and the os being dilated, the forceps was applied, and the delivery was effected after some difficulty. The child was born alive, but was weak, and it took some time to revive it; by using artificial respiration, and allowing a few drops of blood to escape from the cut end of the funis, it was brought round. After the placenta came away very smart hæmorrhage occurred, and continued for some time, requiring firm pressure on the uterus to control it. As there was a great tendency to relaxation, this hæmorrhage seemed to have no influence on the fits. As they continued to recur constantly, the inhalation of chloroform was continued steadily, being only omitted for a short time when the breathing became stertorous. The hair was cut close on one side after some difficulty. As the least touch of the scissors caused twitchings of the muscles, we were unable to cut off the rest of the hair, and were obliged to be content with keeping a bladder of ice applied to the side where we had succeeded in cutting it. Previous to the fits coming on she sometimes opened her eyes; there was then sudden and marked jerking pulsation in the carotids for a few seconds, and then she would go off again into a convulsion. Chloroform was given as in the previous case, and pressed until the breathing became stertorous, when the twitchings of the muscles warned us that a fit was coming on. By this means many fits were prevented, and when they did come on, the intervals between them were gradually longer, until at last an interval of two hours elapsed between the attacks, and then at half-past two on Monday afternoon they ceased altogether, having lasted twenty-three hours. At first water was dashed on the face during the fits, but had no effect in checking them. An enema of assafœtida was given about seven o'clock on Sunday evening, but was not retained for an instant. On Monday morning a bolus of one drop of croton oil and five grains of calomel was placed on her tongue, and washed down with a little water. This acted freely in a few hours, but she was quite unconscious of the action of the bowels until Tuesday morning, when she began to regain

consciousness. The patient's mouth was very dry, and the tongue parched. She sometimes tried to lick her lips; the tongue was not bitten, and in the intervals between the fits small pieces of ice were inserted between the teeth to keep the tongue moist. The chloroform was given steadily by Mr. O'Connor and Dr. Alexander Merrick, who remained at her bedside the entire night, and was kept up until three o'clock on Monday afternoon, when the fits having subsided it was stopped. She inhaled altogether about twelve ounces of chloroform; she had from first to last twenty-six convulsions. This patient was able in a few days to nurse her infant, and left the hospital quite recovered. The urine had been examined several times; at first it was highly albuminous, but this subsided in a few days. There was no trace of albumen when she left the hospital. These two cases resembled each other in some points as regarded treatment, but differed very considerably in others. They resembled each other in one point—namely, that they were cases in which bleeding could not be thought of. In the second case, in which there was smart uterine hæmorrhage after delivery, the bleeding had no effect whatever in controlling the attacks, and left the patient much weaker after it. In this case, as the convulsions came on during labour, occurring at the full period of gestation, and when the os was nearly fully dilated, there was not much doubt as to the course of treatment to be pursued, it being manifest that immediate delivery was the proper plan to adopt; but in the first case, where the convulsions set in at the seventh month, and without any symptoms of labour, the case was very different; and as it is in the treatment of cases of this kind, occurring before labour, that the opinion of even the most eminent obstetricians differs, I have brought this case before your notice, in the hope that it may assist us in coming to some fixed line of practice should we be called on to treat a similar case; and if I may venture to give my opinion as to what ought to be the principles to be kept in view, I would say that if, after using every means possible to check the fits, bleeding and giving tartar emetic in suitable cases, and using aperients and chloroform in those not suitable for depletion, if the fits continue, that we ought to endeavour to deliver the patient, and if labour has not come on, or if on, has not caused sufficient dilatation of the os to use the forceps, we should have recourse to Barnes' dilators, which are easily applied, cause no pain, and are perfectly safe and manageable in their effects on the os uteri; the delivery to be accomplished under chloroform, by the forceps if possible; but if not, then that the use of the perforator is not only justifiable, but absolutely necessary. As the mortality in turning in convulsions is so great as to forbid having recourse to this method of delivery, except in cross births, should the fits recur after delivery, the chloroform should be kept up without intermission as long as a tendency to convulsive action is present, without being at all afraid

to use even very large quantities of chloroform—the effects produced, and not the quantity used, being our guide.

I find a case of puerperal convulsions successfully treated by chloroform, and inductive of premature labour, recorded in *Braithwaite's Retrospect*, Vol. lii., in which twelve ounces of chloroform were used. The inhalation was tried before delivery with some effect, but as the convulsions continued to recur, labour was brought on, and the patient delivered with the forceps. After delivery the chloroform controlled the attacks, and the patient recovered. The case is recorded by Dr. Cuthill. With regard to the treatment of puerperal convulsions, I believe, from the observations of a considerable number of cases, that we have been led very much astray by the prevailing idea that they are always caused by uræmia, and as albumen is nearly always found in the urine in cases of convulsions, this has been put down as being decisive on the point; but from the fact that in cases of recovery from puerperal convulsions the albumen nearly always disappears from the urine in a few days, leaving the kidneys perfectly healthy, and that in many cases, particularly in primipara, there is albumen in the urine during the latter periods of gestation, which does not seem to have any injurious effect on the kidneys after delivery.

I believe the albumen is caused entirely by mechanical pressure on the renal veins. I have seen a great many cases of albuminuria complicated with convulsions, undoubtedly of an uræmic character; and when these patients have recovered, as they frequently do in cases where the seizure is due to acute albuminuria, caused by cold or some such causes, or by the poison of scarlatina, I have always found the urine to remain albuminous for a considerable time after the convulsions had ceased, and gradually to subside as the congestion of the kidneys was relieved by free action of the skin and bowels, thus showing a marked difference between these cases and those of puerperal convulsions, in which I have always found the albuminuria to be but of a temporary duration. Reasoning from this I am of opinion that, in cases of convulsions occurring during gestation, that pressure of a mechanical kind on the abdominal veins by a gravid uterus is the immediate or chief exciting cause of, at least, one form of puerperal convulsions, and the remedy for this form is to empty the uterus and remove the mechanical pressure. Those convulsions which come on after delivery, as in cases following the extraction of the placenta, or which, being caused by congestion from pressure of the gravid uterus which continue after the pressure has been removed by delivery, are either entirely due to reflex actions, or are kept up by irritation of peripheral nerves, reflected to the medulla oblongata and spinal cord. It is in this form of convulsions that chloroform is likely to be most beneficial, but it must be pushed so as to cause the full effects of the drug in paralysing or, at all events, quieting the excited

state of the nervous system, and thereby giving time for the subsidence of the irritation of the peripheral nerves after the immediate exciting cause of the irritation has been removed.—*December* 5, 1870.

Case of Hypertrophy of Heart. By W. C. Townsend, M.D., Senior Physician South Infirmary; Physician to District Lunatic Asylum. Read December 19th, 1870.

Michael Lane, labourer, aged forty, was admitted into the South Infirmary, on November 9th, 1870, labouring under aortic regurgitation.

Patient stated that in January last he first noticed that he was unable to walk up a hill without stopping, a circumstance he attributed to a cough which he had been suffering from for over twelve months. He never suffered from rheumatic fever.

The symptoms on admission were—urgent dyspnœa and cough, the former being so great that he was unable to lie down, and had to be propped up with pillows. He complained greatly at night of a pain and constriction across the epigastrium. A loud double bruit was audible over the base of the heart; the area of cardiac dulness was abnormally increased, the left side of the chest being also fuller and rounder than the right; the pulse was full and bounding.

On November 10th, he was ordered aromatic mixture, "a table-spoonful every four hours, and two galbanum c. colocynth pills to be taken at bedtime. November 11th.—dyspnœa less urgent; was ordered aromatic iron mixture an oz., three times daily, and galbanum and quinine pills at night. On this treatment he continued to improve until December 8th, when he was again attacked with dyspnœa, to relieve which aromatic mixture was ordered, but the symptoms continued until the morning of the 14th, when he sank.

The *post-mortem* examination showed a heart greatly hypertrophied, weighing 36 oz.; the aortic valves were contracted, rigid, and curled up at the edges; the arch of the aorta was greatly dilated, and showed extensive signs of disease; the right side of the heart was engorged, and the lungs greatly congested.

Case of Ruptured Heart. By E. R. Townsend, M.D. Read December 19th, 1870.

Thomas M'Carthy, a labourer, aged sixty-five, was admitted into the South Infirmary for surgical treatment, on December 12th, 1870. He was labouring under Pott's gangrene, from which he began to suffer inconvenience for the first time about a month previous to the date of his admission, though his foot evidenced signs of disease before that period. He was not aware of any injury having happened to him which would account for the disease, with the exception that immediately before the first symptoms an unusual pressure was exercised on the palmar aspect

of the affected foot, at the base of the toes, by an ill-fitting boot. The disease was ushered in by pain, numbness, loss of temperature, muscular weakness in the lower extremity; there was, however, no twitching of the muscles. At the time of his admission he merely complained of diminished temperature and power in the affected part, which presented in a marked degree the symptomatic colour of gangrene; the cuticle of the toes was also commencing to desquamate. With regard to his previous history the patient states, that he never was addicted to alcoholic abuse, or any other habit which would impoverish or contaminate his system. His early life was spent as a labourer. He was in the habit of working much in damp soils. He never complained of suffering any inconvenience from his work previous to the appearance of the disease. He always had a dislike for animal food, and never fed much on a nitrogenized diet.

On the day of his admission he was ordered to take opium internally, in doses of grs. jss., three times a day, while the temperature of the part was maintained by being rolled in carded wool lightly sprinkled on its inner surface with iodine. On December 10th, the same internal treatment was continued, while carbolic oil dressing was substituted for iodine. On December 14th, the patient died suddenly, and, as the *post-mortem* examination showed, from rupture of the right auricle of the heart, the consequence of weakening of the auricular walls. The pericardium was also fully distended by dense coagula of blood. The arteries supplying the affected foot were very tense and impermeable.

Case of Puerperal Mania Associated with Abscess of Left Ovary, pointing externally; Recovery. By J. P. Golding, M.D., Assistant Physician City of Cork General Hospital, and Physician to the City Dispensary, &c.

Mrs. G., a healthy young woman, aged twenty-nine, was confined naturally the 3rd of last February of her third child, and went on very well until the fifth day, when she complained of pain and tenderness in the upper part of left thigh, for which I saw her, and learned that, during pregnancy, she was constantly harrassed with gloomy anticipations and sometimes hysterical, without any hereditary maniacal tendency; the lochiæ had ceased, the secretion of milk was arrested, she was sleepless and restless, with a loaded tongue, and a weak pulse, about 80. A brisk purge, a few leeches to, and stuping of, the part, relieved her, until the evening of the sixth day, when coincident with exquisite tenderness over the lower part of the abdomen, chiefly the left side, she became furiously delirious, expressed herself as having been very wicked, and that demons were in the house ready to snatch her away; to avoid which she would plunge about the bed in a stealthy manner, sometimes so violently as to require considerable restraint. She could not bear to see her husband,

and was continually imploring the priest to save her from the evil spirits. These symptoms became in the progress of the case, though generally persistent, yet alternated by fits of melancholia and distrustfulness. All the time uterine tenderness continued, notwithstanding appropriate treatment by leeches, calomel, and opium, &c., until the fourth week, when there was sufficient indication of the formation of matter over the site of left ovary. At this time Professor O'Connor was called in, and with his valued advice, notwithstanding the persistence of the above symptoms, now associated with great prostration and emaciation, she came to a time, exactly two months from the beginning of her illness, when I opened the abscess in the left iliac region, from which there was an immense discharge of pus, which continued for some time; after which her illusions gradually left her, and she is now quite well.

In the prognosis of this case, I was induced to be hopeful from the little disturbance of the circulation, thus bearing out Dr. Wm. Hunter's opinion.

As to its causes—It has been seen not to be hereditary; although half Gooch's and Burrows' cases were so, and of those which were exciting, I noted only distress of mind and disordered bowels.

Pathologically, different authors have held different views as to its nature; some, as Burns, Davis, Lee, &c., to be a kind of phrenitis, and others, as Gooch, Tyler-Smyth, to be generally caused by exhaustion; and Gooch refers to cases in his book to bear out his views. Other causes are irritation of the bowels and albumen in the urine (Sympson). This latter was constant. I introduce this case to the Society to see how far the condition of the ovary, which I believe to be the seat of the abscess, has contributed to that combination of circumstances described by Marshall Hall, and others as contributing to its production. I find that Dr. West, in his book on *Diseases of Women*, has given some three cases of inflammation of the ovary, in connexion with disordered mind, producing tumours in the iliac region, with discharge of pus through the urethra and rectum; and Dr. Montgomery has stated that he has seen a case of puerperal mania where the ovary was inflamed.

This case is interesting as going to strengthen the opinion of writers such as Burrows, &c., that the cause of puerperal mania is uterine and reflex.

The treatment has been by calomel and opium, iodide and bromide of potassium, and finally cod-liver oil, &c.

PERIODICALS WITH WHICH THE DUBLIN QUARTERLY JOURNAL IS EXCHANGED.

GREAT BRITAIN.

1. The British and Foreign Medico-Chirurgical Review. Churchill.
2. The Edinburgh Medical Journal. Oliver and Boyd.
3. The Retrospect of Medicine. Edited by W. Braithwaite. Simpkin, Marshall, and Co.
4. The Half-yearly Abstract of the Medical Sciences. Churchill.
5. Pharmaceutical Journal. Churchill.
6. The Lancet.
7. The Medical Times and Gazette. Churchill.
8. The British Medical Journal.
9. The Asylum Journal of Mental Science. Churchill.
10. The Glasgow Medical Journal. Dunn and Wright.
11. The Athenæum.
12. The Dublin Medical Press.
13. The Westminster Review. Trübner.
14. Transactions of Obstetrical Society. London: Longmans.
15. Journal of Cutaneous Medicine and Diseases of the Skin. Edited by H. S. Purdon. Churchill.
16. The Practitioner; a Monthly Journal of Therapeutics. Macmillan and Co.
17. The Journal of Anatomy and Physiology. Macmillan.
18. The Food Journal. London: J. M. Johnson and Sons.

INDIA.

19. The Indian Annals of Medical Science. Calcutta: Barham, Hill, and Co. London: Longmans, Green, Reader, and Dyer.
20. The Madras Monthly Journal of Medical Science. Madras: Gantz, Brothers. London: Hardwicke.
21. Indian Medical Gazette. Calcutta: G. Wyman and Co.

AUSTRALIA.

22. The Australian Medical Journal, Melbourne: Stillwell and Knight. London: H. Baillière.

AMERICA.

23. The American Journal of the Medical Sciences. Edited by Isaac Hays, M.D. Philadelphia: Henry C. Lea. London: Trübner and Co.
24. The American Journal of Science and Arts. Conducted by Professors B. Silliman, and J. D. Dana, &c. New Haven: Editors.

AMERICA.—*Continued.*

25. The American Journal of Insanity, Utica, N.Y. State Lunatic Asylum.
26. The American Journal of Obstetrics and Diseases of Women and Children, New York: W. A. Townsend and Adams. London: Trübner and Co.
27. The Cincinnati Lancet and Observer. Cincinnati: E. B. Stephens, M.D.
28. Canada Medical Journal. Montreal: Dawson, Brothers.
29. The New York Medical Journal. New York and London: D. Appleton and Co.
30. The Medical and Surgical Reporter. Philadelphia: S. W. Butler, M.D.
31. The Richmond and Louisville Medical Journal. Louisville, Ky.: E S. Gaillard, M.D.
32. The Medical Record. New York: Wood & Co.
33. The New Orleans Journal of Medicine. New Orleans: W. S. Mitchell, M.D. London: Trübner and Co.
34. The American Practitioner. Louisville, Ky.: John P. Morton and Co. London: C. D. Cazenove.

FRANCE.

35. Gazette Médicale de Paris. Paris
36. Gazette Hebdomadaire de Médecine et de Chirurgie. Paris: Victor Masson.
37. Journal de Chimie Médicale, de Pharmacie, de Toxicologie, et Revue denouvelles scientifiques, nationales et étrangeres, &c. Paris: Labé.
38. Journal de Médecine de L'Ouest. Nantes: Mellinet.
39. Journal de Pharmacie et de Chimie, &c. Paris: Victor Masson.
40. L'Union Médicale. Paris.
41. La Lancette Française, Gazette des Hôpitaux civils et militaires. Paris.
42. Revue Médicale Française et étrangère. Publié par le Docteur Sales-Girons, Paris.
43. Archives Générales de Médecine. Paris: Asselin.
44. Bulletin de l'Académie Impériale de Médecine. Paris: Baillière.
45. Revue de Thérapeutique Médico-Chirurgicale. Paris: Dr. A. Martin-Lauzer.
46. Journal de Médecine et de Chirurgie Pratiques a l'Usage des Médecines. Par Lucas-Championnère. Paris.

THE

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MAY 1, 1871.

PART I.

ORIGINAL COMMUNICATIONS.

ART. XVI.—*The Dependency of Life on Disorganization.* By HENRY FREKE, M.D., Univ. Dub., M.R.I.A.; Fellow and Ex-Vice-President, Censor, and Examiner in Medicine, King and Queen's College of Physicians in Ireland; Senior Physician to Dr. Steevens' Hospital, Dublin; Physician to Swift's Lunatic Asylum; Professor of the Practice of Physic, and Lecturer on Clinical Medicine in Steevens' Hospital Medical College, &c., &c.

> "The fundamental inquiries in physiology ever must be into the nature of life and its relation to death. I have attempted to point out what I regard as the nature of life, and have defined living to be—the act of being elevated in the scale of organization. . . . . I have endeavoured to show what I conceive to be the nature of death and its relation to life, pointing to the nature of the essential dependency of the latter upon the former."
> —H. FREKE, in *Medical Times* (London), 3rd February, 1849, Vol. xix., page 296.

THE subject of "The Dependency of Life on Disorganization" being one which I understand is at the present moment occupying the attention of physiologists in America, I feel it has become incumbent on me to make a few observations on that question.

Some time in 1869 there reached my residence, anonymously,

through the American post, a document reprinted from the *St. Louis Medical and Surgical Journal*, entitled "Dr. Watters' Doctrines of Life." The extract comprised a letter addressed by Mr. James Hinton, of London, to the editor of the *St. Louis Journal*, and criticisms on that letter by Dr. Watters. Owing to circumstances, the detail of which would occupy much space, and be interesting to none, I have been prevented having the advantage of perusing Dr. Watters' criticisms till the month of March of this present year 1871. Hence has arisen this unavoidable delay of my notice of Dr. Watters' criticisms. I have not lost a moment at my disposal since the perusal of Dr. Watters' remarks in arranging the following observations. Mr. Hinton's generous letter, which had previously appeared in a former number of that Journal, I was already, through the courtesy of the editor, possessed of.

The subject under discussion in the paper alluded to is "The Dependency of Life on Disorganization," a topic in which I am personally much interested, being myself, I believe, the first to advance it in 1848; and conceiving it to be a question of importance, not merely to physiological science, but also to practical medicine, it occurred to me that a paper on the subject might not be uninteresting to the readers of the *Dublin Quarterly Journal*. It is not, however, my intention—nor would it be suitable—in this Journal to discuss Dr. Watters' criticisms in detail. I shall reserve that for a more appropriate place. I shall, however, here point out what I regard as the prominent and prevailing error in those criticisms before giving elsewhere a detailed analysis of the entire. In order to illustrate my own views, I shall introduce what I regard as a *most erroneous* analogy attempted to be instituted by Dr. Watters between the movements of a clock and the phenomena of life, no such analogy as that attempted to be established existing. I shall analyse Dr. Watters' analogy minutely for a twofold reason—namely, first, because it will afford me a convenient channel whereby popularly to *illustrate* my own physiological views; and, secondly, because it will enable me with facility to point out what I conceive to be the grave error into which Dr. Watters has fallen, and against which I wish to warn physiologists.

I desire, however, at the outset emphatically to state that my object in now writing is not to lay claim to priority of views or identity of thought with Dr. Watters or with any man living. With the views entertained by Dr. Watters I am but very

imperfecty acquainted, and from what I can collect of them from his criticisms, I believe them to be erroneous. As to myself, I have long since become indifferent who *now* takes possession of my thoughts, and leave the decision as to their true source to posterity.

But I write because, on perusing Dr. Watters' remarks, I felt it had now become incumbent on me in justice to Mr. Hinton, even irrespective of myself, to make known what those views *really are* which have been so generously recognized by Mr. Hinton, and to rescue them from the misrepresentation and error with which hitherto they have been ever associated. I am desirous, moreover, of recording in Ireland the flattering opinion expressed of them in America by Mr. Hinton. It is right I should here state that Mr. Hinton is a gentleman I have never had the privilege of personally meeting, and one to whom I am entirely unknown except through my writings, and I would avail myself of this opportunity to express my sense of that gentleman's generous, disinterested, and spontaneous support.

In his letter to the editors of the *St. Louis Medical Journal*, 28th August, 1868, Mr. Hinton, in alluding to my publication of 1848, expresses himself in the following flattering terms :—

"I am very happy to draw the attention of Dr. Watters and your readers to the third person to whom reference is made in my volume as having published, before me, the doctrine of the dependence of life on decomposition. I will quote a few of his words: 'Death is essentially a part of life. It is the transit of the organizing influence from the organizing atom which causes that atom's death. It is the transit of the same organizing influence to that atom's type which gives to that type its life. But it is during, and alone during such transit, that conjointly the two atoms live. . . . Thus are two opposite processes, of necessity, concerned in producing the phenomena of actual life, . . . and thus, too, it becomes apparent how death is a part of life.' Much more to the same effect might be quoted, but I doubt not that these sentences are enough, and that behind the peculiar phraseology (connected with other ideas which I need not here detail) the identity of thought will be recognized at once.

"But these words were published in 1848, and their author is Dr. Freke, of Dublin.[a]

"It gives me the greatest pleasure, in which no one, I am sure,

[a] Freke on Organization, Dublin, 1848.

will more heartily share than Dr. Watters (who in the world of thought is wealthy enough to be generous) to do this act of justice to a discoverer to whom we owe more than can yet be known, but whose only reward hitherto has been neglect. Even I, who came so long after, am (thanks to Dr. Watters) a little known in connexion with this grand thought; but the man upon whom, so far as I am aware, it *first* dawned, and who devoted great powers and great labour to its development, has received no acknowledgment of its value. It is a matter for regret and for rectification. I said he had received *no* acknowledgment, but perhaps I was wrong. He had received that best acknowledgment, of seeing it caught up and borne witness to by other minds, and has that best reward—the consciousness of work well done, of seed laid up for future fruitfulness. Your readers will with pleasure recognize in him an instance of *dormant vitality*, on which they can test the value of the opposing theories.

"I have forwarded to Dr. Watters a copy of a small volume published by Dr. Freke in 1861, 'On the Origin of Species,' with the view of calling attention to views so long previously put forth by him.

"It gives me great satisfaction to make this feeble attempt to do justice to a citizen of a sister-isle; but I rejoice still more to recognize in these successive flashes of the same idea, first in Ireland, then in America, then in England, proofs that the idea itself has its origin in truth, and is written deep in nature.

"I am, Gentlemen, your obedient servant,

"James Hinton."[a]

Before explaining the meaning of some of the expressions in the quotations from my publication introduced by Mr. Hinton—expressions which, in the absence of their context, require explanation, I feel called on to make the following observation. In the present day an undue and, I think, an unjust weight is given to

[a] Although not strictly in place *here*, I may be allowed to observe that Dr. Watters emphatically denies any identity of his views with mine. Dr. Watters is perfectly correct; our views are very different indeed. But this in no way militates against the correctness of Mr. Hinton's statement. Mr. Hinton throughout his letter alludes to but *one* solitary thought—namely, the dependency of life on decomposition. No other thought is referred to in the letter. All Mr. Hinton asserts is that in 1848 I entertained *that* thought. In this statement Mr. Hinton is *most unquestionably correct*. If Dr. Watters does *not* entertain *that* thought, Mr. Hinton appears to have inadvertently fallen into the error of supposing he did.

microscopic researches. No one is more fully aware than I am of the immense value of the microscope to physiological science, but it is not *everything*, and ought not to be regarded as such. A man may be an experimenter without being a microscopist. An experimenter, as I take it, is one who interrogates nature, and nature admits of being interrogated in a variety of ways. The mind is the interrogator, and facts reach the mind through various channels. Nature is frequently interrogated by the mind as efficiently *without* as with the microscope. I have used the microscope so extensively as to have injured my vision, and be compelled to abandon its use, and yet I have rarely seen much beyond the very thing that I sought for. If I by my mind, unaided by the microscope, reach a certain result, and announce that result to the world, is the man who searches for the same in the microscope and finds it—which I am myself incapacitated from doing—to be regarded by the world as its discoverer? Such is *fact*, but not *justice*.

I shall say a few words in explanation of my terms. I employed the words "organizing atoms" in 1848 to express the ultimate organizing molecules existing in organic nature. I desired to convey by that expression *pretty nearly* the same idea as that *now* intended to be expressed by the recently introduced word "Microzyme," if I correctly understand what the term is designed to express. I used the terms "*organized residual product*" to express the organic structure developed or organized by the organizing atom, meaning thereby to convey what has subsequently been expressed by the terms "formed material," if I correctly understand that expression. I shall illustrate *my own* meaning in both those expressions—viz., "organizing atom" and "organized residual product."

An organizing atom of the *lowest* degree of organization[a] converts inorganic elements into an organic substance—say, for the purpose of illustration, into albumen. This albumen is one example of what I meant by an "organized residual product." A second organizing atom, or microzyme (name it as you please), higher in the scale of organization than the former, elevates albumen to the condition of *muscle*. Muscle is another example of what I meant by "an organized residual product," subsequently named "formed

[a] This atom of the *lowest* degree of organization I *subsequently* called "*Georgat*," as the atom which organizes *earth*, forming therefrom what is now named "Protoplasm," if I correctly understand the meaning of that term.

material," if I correctly understand the meaning of that term. Now observe, *while* the process of *raising* inorganic elements to the condition of albumen, and that of elevating albumen to the condition of muscle, *while* I say these two processes are in progress, matter is living and is being raised in the scale of organization. Hence I ventured in 1848 to submit as a definition of "*Living*"—"the act of being elevated in the scale of organization."

Having in the early part of my publication *contrasted* the forces, heat, gravity, &c., which in the inorganic world put *masses* of matter in motion, with those forces which in organic nature give rise to those *molecular* movements upon which all vital phenomena are dependent, I ventured to submit the expression "organizing agency" to express a force in organic nature analogous to what I termed "its analogue caloric" in the inorganic world. So that, as in the inorganic world, we see heat opposing or antagonizing gravity—the one repelling, the other attracting—the one *elevating*, the other *depressing*—and the two, *by their antagonistic action*, keeping masses of matter in motion. So, in the organic world, I ventured to submit there is to be recognized a force (which I termed "organizing agency") *antagonizing* chemical force, and that these two forces, *by their antagonistic action*, give rise to those molecular movements upon which all vital phenomena are dependent. In instituting this analogy between an "organizing agency" and what I termed "its analogue caloric," I emphatically intimated that I expressed *no opinion* as to which hypothesis most satisfactorily explained the phenomena of heat.

I used the terms "influence" and "agency," as applied to the *cause* of organic phenomena, simply because they appeared to me to be the *least theoretic* expressions I could select for that purpose, inasmuch as they imply nothing whatever as to the *nature* of the organizing cause. I distinctly expressed that any inquiry into the *nature* of such cause was, in my opinion, "at variance with a rational philosophy." Let us inquire into the meaning or purport of the terms "influence" and "agency." When I state that I *influence* a man's conduct by argument, I express no opinion on the nature of argument. And when I say that the *agency* I employed to convince that man was my reason, I give no opinion as to the *nature* of reason. Is *any* effect produced without the operation of *some agency?* And am I to be told that *organization* is an exception? Am I to be told that organization takes place

without the operation of any *agency* whatever ? If, then, an *agency* be required to effect *organization*, wherein consists the impropriety of designating it—an "organizing agency ?" I have reflected much on this problem, and the only solution I can reach is that the expression "organizing agency" and the others used by me were submitted by an Irishman, and no good thing can come out of Ireland.

With this explanation I shall now introduce Dr. Watters' analogy between the motions of a clock and "*all* the various 'vital' motions or processes." I introduce this analogy for the twofold purpose I mentioned—namely, that of affording me an easy vehicle for a popular illustration of *my own* physiological views, and at the same time enabling me, with facility, to point out what I regard as the great error in Dr. Watters' views. In "attempting to explain the phenomena of the clock," Dr. Watters observes as follows:—

"I would say that gravity or downward tendency of the weights is the *logical antecedent* to *all* the various motions of the clock; that the motions of the clock are logically *consequent* upon the descent of the weights, and that the *speciality* of the motions of the clock, indicated in its keeping time, is due to the adjustment, to the arrangement of parts, to mechanism. And, as there is no time indicating agency 'radiating' from the weights, so I would say that decay or disintegration, or a 'downward' process, is the logical antecedent of life, with which all the various 'vital' motions or processes are logically *consequent*—the speciality being determined by form or organization. Is there any ambiguity in that? Can any one misunderstand it, or confound it with any other 'thought' not identically the same ?" *

In reply to Dr. Watters' interrogatories at the conclusion of this paragraph, I would answer *most certainly not.* Nothing could be clearer or less ambiguous than each statement in that paragraph; but nothing, in my opinion, could possibly be more erroneous than every one of them. I will endeavour to *convince* Dr. Watters—and that without either parody or satire—that every statement in that paragraph is an error. I will endeavour to *convince* Dr. Watters that his views on this subject are altogether

* Dr. Watters has introduced two other analogies—namely, that of a lever and that of lead placed upon water. However, as identically the same error pervades all three, the same argument is applicable to all. I shall consequently *here* confine my observations exclusively to the analogy of the clock. Elsewhere I shall analyse each of Dr. Watters' illustrations in detail.

erroneous, and that his imaginary analogy has no real existence, and will further attempt to point out at considerable length what the *real* analogies are which admit of being recognized between the movements of a clock and the phenomena of life.

In justice, however, to myself I must, at the outset, premise that one who can recognize any very close analogy between the rough motions of a clock—the creation of man—and those wonderful molecular changes upon which vital phenomena depend, does not contemplate physiological phenomena from the elevation from which I should desire to survey the creations of God. Analogies between the various works of the Creator are not only legitimate but wise, because in such works *everything is perfect;* consequently a knowledge of one law of God may by analogy lead to a recognition of another (hence my "*radiation*" of an "organizing agency" of which by-and-bye). But any analogy between God's works and the constructions of man must necessarily be defective, and often lead to erroneous conclusions.

From the foregoing quotation I collect that Dr. Watters entertains altogether erroneous opinions relative to any analogy which may be said to exist between the movements of a clock and the phenomena of life. The mechanical movements of a clock, *while indicating time*, can at best be said to bear but a loose analogy to the molecular movements of life, and that analogy, such as it is, has relation *exclusively* to the phenomena of *animal* life, and no relation whatever to the phenomena of *organic* life. There is not the slightest analogy between the phenomenon "*indicating time*" and the phenomena of *organic* life. The failing to recognize this fact appears to be the error into which Dr. Watters has fallen in *his* analogy between the motions of a clock and those of life. In his illustration he appears to have confounded together two things which in nature are entirely distinct—namely, *organic* and *animal* life. He appears to ignore altogether *organic* life as a class of phenomena distinct from those of *animal* life. To ignore the fact that there must be a *constructive* as well as a *destructive* life; that there must have been a building up or organizing *before* there could be a pulling down or disorganizing. In a word, he has ignored the fact that elevation must *precede* descent, both in the physical and in the physiological world.

For the process whereby these elevations respectively are affected —namely, in the physiological world by organic life, and in the physical world by mechanical labour—for such process Dr. Watters

substitutes the words, "adjustment," "arrangement of parts," and "mechanism," as if those adjustments, arrangement of parts, and mechanism constituted no part of life. Why!—it is these very adjustments, arrangement of parts, and mechanisms which constitute the entire phenomena of *organic* life. Do the raw materials of a clock adjust themselves spontaneously? How, then, are these adjustments, &c., attained? In the physiological world they are attained by the process of *organic* life; in the physical world by the analogue of *organic* life—namely, mechanical labour. Organic life does for the physiological world what the artisan does for the world of mechanics. The former *adjusts* and *arranges the parts* of living beings; the latter gives its *mechanism* to the clock.

Now, what I desire to impress is this, in both worlds—namely, that of life and that of mechanics—the adjustment and the arrangement of parts must have *preceded* the working of the machine. The clock must be manufactured before it can indicate time, and in man an *organic* life, for the construction of his brain, must have *preceded* the thinking of that brain, which is its animal life. *Thinking* is what constitutes the so-called *animal* life of the brain. Thinking—namely, the development of thought or animal life of the brain—takes place simultaneously with the disorganization and *destruction* of that brain which must have been *previously* constructed and organized by *organic life.* In a word, the *construction* of the brain must have *preceded* its destruction.

As Dr. Watters appears to me to ignore organic life altogether, I shall say a few words on the nature of the distinction between organic and animal life, and point out in what way they are related, and how each has its analogue in the clock. I will endeavour to show how organic or constructive life is analogous to the manufacturing of the clock, and animal or destructive life is analogous to the functions of the clock—namely, to *the indicating of time.*

What, then, is the true distinction between *organic* and *animal* life? It is this—organic life is *constructive* life; animal life is functional life, and is *destructive.* Organic life forms, constructs, or develops organic tissues, and does no more. This it does alike in the vegetable and in the animal kingdom. In the former it *constructs* wood, &c., and does no more; in the latter it *constructs* brain, &c., and does no more. With the *construction* of the organic tissues their *organic* life ends. *During* this constructive process—say of the brain—matter is in the act of being elevated in the scale of organization from inorganic elements to the condition of brain,

consequently, during the entire of that elevating process matter has been "*living*," first, in the vegetable kingdom, and secondly, in the animal, but in *both* it is *organic* or *constructive* life. When the brain has been *constructed* its *organic* life is at an end. Animal life, on the contrary, is *functional*, and not *constructive*. Animal life employs the tissues (brain, &c.) constructed by organic life. Animal life is functional, and is *destructive*. Animal life breaks down and disorganizes the organic structures organized by organic life. *Organic* life is analogous to the *constructing* of a machine, say a clock. Animal life is analogous to the *working* or *function* of that machine—namely, the *indicating of time*. *During* the destructive process of animal life matter is descending in the scale of organization (like the weights of a clock), and during such descent is giving development to *immaterial phenomena* (in contradistinction to *material* organic structures—viz., the result of *organic* life), such as the phenomenon of *thinking* by the brain, and the analogous phenomenon, *indicating time*, by the clock.[a] In a word, *organic*

[a] It is right, I should here observe, that certain able physiologists, whose opinion on any question in physiology must be received with respect, and demands every consideration, have recently maintained that the animal tissues, brain, muscle, &c., do *not* undergo disintegration while discharging their functions, but that those tissues and the blood stand towards each other in relations analogous to those subsisting between the steam-engine and the steam. I could not stop here to analyse this opinion, which to me, notwithstanding the high authority of its supporters, appears unphysiological and untenable. The experiments on the subject have not shaken my faith in the old opinion, first advanced, I believe, by Liebig. To me it would appear to admit of no question that the animal tissues, brain, muscle, &c., in discharging their functions undergo such molecular change—be it called disintegration or otherwise—as renders nutritive restoration or repair *indispensable*. Otherwise wherefore the necessity for rest? The tissues do not, doubtless, undergo complete disorganization at once; they are protected from such by the nutritive process. But if the process of nutrition were suspended, each of those tissues, when *once* its functional capacity had been completely exhausted, would be so far reduced in the scale of organization as to be utterly incapacitated for a repetition of that function; consequently, as brain or muscle, it would be physiologically, if not actually anatomically, disorganized. Why, otherwise, would the brain require sleep, the voluntary muscles repose, and each cardiac rhythm its period of pause? Why, with an adequate supply of food, are we not able to work our brain, muscles, &c., for an *indefinite* period, like a steam-engine with an adequate supply of steam? Because the tissues are disintegrated, and require nutritive repair. If the animal tissues did *not* undergo disintegration during the active discharge of their functions, why should not the animal, like the vegetable, continue to increase in dimensions during the entire period of its organic existence? It is because the organic tissues developed by the vegetable do *not* undergo disintegration when their construction has been completed, that the vegetable continues to grow and increase in dimensions during its entire life. Such is not the case with the animal, and that for this reason, namely, when the construction of the animal tissues, brain, muscle, &c., is completed, those tissues undergo disorganization while discharging their functions. I could not

life developes *material* organic structures, such as brain; animal life developes *immaterial* phenomena, such as thought. Thus, then, the brain of man may be said to have two lives—an organic life for its construction, an animal life for the development of thought. Let us analyse these two lives of the brain more closely, and I shall be able to point out, at one and the same time, the purport of the views published by me in 1848, and the analogies between the two lives and a clock. First, then, the brain of man must have been constructed before it can think, and consequently has two lives—viz., an organic or constructive life for its development, and an animal or functional life for thought.

Let us analyse these two lives of the brain more minutely. First, then, the *organic* life of the brain. In the organic or constructive life of the brain are to be recognized (at least[a]) two stages—the one in the vegetable, the other in the *animal kingdom*—but *both*, notwithstanding merely stages in the *organic* or constructive life of the brain, and quite distinct from its *animal* or functional life. The two stages in the *organic* life of the brain are these: namely,

1st, The elevation of inorganic elements by a vegetable organizing atom or germ to the organic condition (say) of albumen; and

2ndly, The raising of that albumen by an animal organizing atom or microzyme (name it as you please) to the more highly organized condition of brain. It will be observed, that this second stage in the *organic* or constructive life of the brain, although it takes place in the *animal kingdom*, is quite distinct from the so-called *animal* life of the brain—that is to say, distinct from its functional life of developing thought. This latter—as we shall presently see—depends upon a distinct and opposite class of phenomena.

The *factors* (to borrow a term from mathematical science), then, in the *organic* life of the brain, are the following five, viz.:—1st, The inorganic elements of the earth (these are the analogue of the raw materials of the clock). 2nd, A vegetable organizing atom or

dwell longer on the subject now, but were it otherwise I think I could convince physiologists that the old is the true physiological view. My object in introducing the subject in the text is to contrast this process of disintegration of the animal tissues with the analogous process in a clock while its weights are descending, and where there is no nutritive repair to protect from complete disorganization, namely, from complete loss of functional capacity, until the weights have been again raised.

[a] I say "at least" two, because I believe there are more than two stages in constructing the brain, but *two* is all that is here required for my purposes of illustration and analogy.

germ which *imparts* organization to those inorganic elements, and converts them into an organic structure, say albumen (this organizing atom or germ is the analogue of the artisan or clockmaker). 3rd, The organic structure, viz., albumen—formed by the "organizing atom" in the vegetable kingdom (this albumen is the analogue of the clock when it has left the maker's hands, but *before the weights have been would up*). 4th, An animal organizing atom or microzyme (name it as you please) which confers increased organization on albumen—viz., raises it in the scale of organization, converting into brain (this is the analogue of the *winder* of the clock); and 5th, The brain.

The fifth factor in the organic life of the brain is the organized residual product—the brain. But mark brain *before* its function of thinking has commenced; brain *before* it has as yet received a stimulus to action from the sensorium; brain *before* the equilibrium between the forces which keep it organized has been disturbed. The moment that disturbance takes place the *animal* life of the brain commences. The brain becomes a *thinking* organ. The *first* life, namely, the organic or constructive life of the brain is now completed. The brain is now in a condition *somewhat* analogous to that of a vegetable which has completed its growth. I say somewhat analogous, for the condition of the brain, and that of a full grown vegetable, are by no means identical. There is this marked distinction between them:—the brain is now possessed of its "potential energy" for the development of *animal* phenomena (viz., thought), the vegetable possesses no *such* potential energy. The *organic* life of the brain is now completed. The brain *now* is the analogue of the clock that has been wound up, but from which the *pressure on the winding key* has not as yet been *withdrawn*. It is in the condition of a clock, as we shall presently see, whose weights have been raised by some force, but from the action of which force those weights have not as yet been emancipated, and are consequently still fettered by the opposing forces—gravity and the force which raised them. The two forces retain them in a condition of static equilibrium, and consequently there is *no* downward motion of the weights till that force which raised them is *withdrawn*. So, too, the brain, when its organic or constructive life is completed, will remain ever inactive till its condition of static equilibrium is disturbed by the stimulus of a sensation from the sensorium. Then, and not till then, its animal or functional life commences.

Before proceeding to an analysis of the animal life of the brain, I would ask the reader to recognize in the preceding my meaning in three of the expressions employed by me in 1848, namely, "organizing atoms," "organized residual products," and my definition of "living." In the *organic* life of the brain are to be recognized two "organizing atoms," and two "organized residual products." The organizing atoms are, first, the germ—if that term be preferred—which, in the vegetable kingdom, raised inorganic elements to the condition of albumen; and second, the microzyme—if that term be preferred—which, in the animal kingdom, raised albumen to the condition of brain. The two organized residual products—or formed material, if that term be preferred—are albumen and brain.

The definition of "*living*," I ventured to submit in 1848, was this, viz., "the act of being elevated in the scale of organization." *While* the inorganic elements were being converted into albumen they were living—they were in the act of being elevated to the organic condition. *While* albumen was being raised to the organic condition of brain it was *living*, it was in "the act of being elevated in the scale of organization." The brain was then *acquiring* what is now termed its "*potential energy*," to enable it subsequently to manifest its "*actual energy*"—thought. When the elevation of brain is completed its *organic* life is ended.

There is another topic to which the present is the appropriate place to refer, namely, what I meant to convey by the expression "the dependency of life upon death." The passages quoted by Mr. Hinton from my publication of 1848 related *exclusively* to the phenomena of *organic* life, and had no reference whatever to the phenomena of *animal* life. What I ventured to submit in relation to "the dependency of life upon death" was this, that the formation or construction of all organic tissues—*vegetable* and *animal*—which is accomplished by the process of *organic* life, is dependent on the disorganization and death of the organic *germ* (if *that term* be preferred), or, to use my own expression, dependent on the disorganization and death of "the organizing agent or atom," by which those organic tissues are developed. This is what I advocated several years before the distinctions between "germinal matter" and "formed material" had been submitted to the public.

Such is what I advocated in 1848 as "the dependency of life upon death." Such is obviously *not* what Dr. Watters subsequently advocated, inasmuch as, *at the present*, his analogies refer exclusively

to the phenomena of *animal* life, and have no relation to those of organic life, to which latter, exclusively, I referred.

Let us now turn to the *animal* life of the brain. What is *animal life?* It is the development of *immaterial* phenomena by *material* organic tissues. Those immaterial phenomena—such as thought—which constitute animal life are developed by the disorganization of the organic tissue which develops them, namely, by the destruction of the tissues constructed by *organic* life.

The animal life of the brain is the development of thought, and the immaterial phenomena—*thinking*—is dependent on the disorganization of the brain.

The molecular motion set up amongst the components of cerebral cells (caused by a sensation from the sensorium, or by an idea from other cells), terminates in the disorganization of those cells, and during the continuance of this molecular motion, the development of thought is going on. Thus then *simultaneously* with that molecular motion, which terminates in the disorganization of the brain cells, those cells develop the phenomena of thought, mark distinctly *simultaneously* or *synchronously* with the process of disorganization, and neither before or after that process. What then becomes of those mysterious "*logical* antecedents, with which *all* the various vital motions or processes are logically *consequent?*" But of this presently.

Let us return to the analogy of the clock. In the language of analogy, the clock too—like the brain—may be said to have its two lives. First, an organic life which *constructs* it, making it an instrument *competent* to indicate time; and, secondly, a (so to speak) *animal* life which makes it a time-indicating instrument, makes it an *indicator* of time.

The clock—like the brain—must have been constructed; but it must have been not only constructed but also *wound* before it can *work*. The *constructing and winding* of a clock conjointly constitute its (so to speak) *organic* life; its working—namely, its *indicating time*—may be said to bear some analogy to *animal* life. Let us analyse these two (so to speak) lives of the clock—organic and animal—to ascertain their relations and analogies.

In the *organic* life of a clock—like that of the brain—are to be recognized two distinct stages, viz.:

1. The elevation of the raw materials by the clock maker to the mechanical or *organic* condition of a clock; and

2. The raising of that clock in the scale of organization by winding up the weights.

The former—namely, the manufacturing of the clock out of raw materials (viz., the *first stage* of its *organic* life), may be said to be analogous, in the organic world, to the conversion of inorganic elements into an organic substance such as albumen.

The latter—namely, the winding of the clock or raising of the weights (viz., the *second stage* of its *organic* life), is analogous to the elevation of albumen in the scale of organization to the condition of brain.

These two stages—the *manufacturing* and the *winding* of the clock—complete the (so to speak) *organic* life of the clock. Every action of the clock subsequent to the raising of the weights must have relation to its (so to speak) *animal* life. Thus, then, as in order to construct the brain—namely, an organic structure *competent* to think—two things were necessary—namely, first to raise inorganic elements to the organic condition of albumen; and secondly, to elevate albumen to the organic condition of brain, viz., an organic structure *competent* to think or give development to the *animal* phenomenon, *thought*. So, in like manner, in order to construct a clock—namely, an instrument *competent* to indicate time, two analogous requisites are necessary—namely, first, to raise the raw materials to the organic condition of a clock; and secondly, to raise the weights of that clock whereby it is rendered an instrument *competent* to indicate time, a function, it may be loosely said, analogous to the animal function, *thinking*. Let us contemplate the condition of the clock when its organic life has been completed and *before* its (so to speak) animal life has commenced. It is this: The clock has been wound; the weights have been raised by the muscular force of the winder, but the hand that turns the key has not as yet been *withdrawn*. The pressure is still maintained on the winding key. The weights are still retained unmovable in their upward position. The moment the hand is *withdrawn* from the winding key the (so to speak) *animal* life of the clock commences. But *before* the pressure is withdrawn from the key the condition of a clock is somewhat analogous to a vegetable that has completed its full growth. It is analogous to the brain *before* that brain has as yet received a stimulus from the sensorium whereby its animal function of thinking is called into action.

Both the brain and the clock are in working *order*, but are not *working*. They are *competent* to discharge their respective functions, but are not as yet discharging them. They are possessed—

to the full—of "*potential* energy," but as yet manifest no "actual energy." We shall presently inquire how the clock's (so to speak) *animal* life is *caused to commence*—that is to say, what must be done to make the clock *begin* to indicate time. But first let us recapitulate the foregoing.

The organic life of the clock then—like that of the brain—comprises in it five distinct factors:

1. The raw materials from which the clock is constructed, analogous to the inorganic elements from which albumen was formed.

2. The artisan, or clock maker, analogous to the organizing atom or germ (name it as you please) which converts inorganic elements into albumen; which converts inorganic elements into "an organized residual product," or "formed material" (name it as you please) known as albumen.

3. The clock completed as it has left the maker's hand, analogous to albumen in the organic world.

4. The hand or other force that winds the clock, analogous to the organizing atom or microzyme (name it as you please) which raises albumen in the scale of organization to the more elevated condition of brain.

5. The clock wound up, analogous to the brain when its organic construction has been completed, but *before* its function of thinking has commenced. Before it has received its first stimulus from the sensorium, whereby it is *caused* to develop an *idea*. This completes the *organic* life of the clock. All action after this pertains to its (so to speak) *animal* life.

Here again may be recognized the meaning of the expressions "organizing agent" and "organized residual product" used by me in 1848. Is not the artisan or clock maker an organizing agent? Does he not *impart* to the raw materials the organization of a clock? Is not the clock an *organized* machine, and when completed is it not the residue produced, and consequently "an organized residual product?" I used the expression "organized residual product" (corresponding, I believe, with the subsequently introduced expression "formed material") in contradistinction to "organizing agent or atom," which forms the residual product and reproduces its own type.

Now, what is the real distinction between the artisan who confers the organization of a clock upon the raw materials upon which *he works*, and the organizing agent or microzyme (name it as

you please) which confers organization upon *its* raw materials—namely, inorganic elements? It is this: In the artisan the cerebral cells which *conceive* the construction of the clock, and which, by the exercise of *volition*, impart the required motions to his muscles—these cerebral cells, *while conceiving and willing*—both mental actions—are discharging functions of *animal* life, and are themselves undergoing disintegration and disorganization while performing those mental functions; whereas the organizing agent or microzyme (name it as you please) which confers organization on inorganic elements, while conferring this organization is discharging a function of *organic* life, and during the discharge of that function is itself undergoing the process of disorganization, is *dying*. Hence "the dependency of life upon death." Thus, then, the constructing of the clock by the clock maker, and the raising of the weights by the hand of the winder—these *two conjointly* constitute the (so to speak) *organic* life of the clock. While the weights have been rising—that is to say, while the clock has been in "the act of being elevated in the scale of organization," *two* forces have been acting on those weights—two opposing or antagonistic forces—namely, the force that raised them in opposition to gravity, and gravity its opposing force. During their ascent the raising force has the mastery over gravity. It is only so long as the raising force maintains this mastery over gravity that the weights are *rising*. Then, and then *only*, the clock is "*living;*" for then, and then only, it is in "the act of being elevated in the scale of organization" (I say then *only* is *living*, excepting, of course, when it was being constructed by the artisan—namely, in its *first stage* of organic life). When the weights have reached their greatest elevation, like their analogue the brain, all motion in either direction, upwards or downwards, is for the time at an end. All has again become still. The two opposing forces now balance each other into a condition of static equilibrium. Neither has the mastery over the other. The weights are at rest. A disturbance of this balance in *either* direction again gives motion to the weights. If the raising force be *increased*, there will be an upward motion such as the raising of albumen to the condition of brain. If the raising force be withdrawn, there will be a downward motion such as the descent of the weights of the clock. But mark distinctly, the raising force must be withdrawn *before* the motion of the weights can commence; consequently "gravity or the downward tendency" is *not* "the logical antecedent to *all* the

various motions of the clock," inasmuch as *antecedent* to the action of gravity must be the *withdrawal* of the raising force. If the raising force be withdrawn, the weights now unfettered by opposing forces become amenable to the laws of gravity, and begin to descend.

Thus, then, when the force which raised the weights—say the hand which wound the clock—is *withdrawn* from acting on the weights, then, and not till then, the weights begin to descend. The clock begins to indicate time. I shall have to return to this presently, but would now observe that, *while* the clock is indicating time, its weights are descending, and, in the language of analogy, it is dying and not living; when it has ceased to indicate time it is down and is *dead*. While dying (viz., while *parting* with its organization), that clock has been indicating time, and such indicating of time may, as I have observed, in a loose way, be said to be analogous to the animal phenomenon, *thinking*, but has no analogy whatever to the *constructive* phenomena of organic life. Bear, then, in mind that while the clock is indicating time its weights are descending, and that clock is *parting* with all the organization which has been imparted to it by the winder. In the language of analogy, it is undergoing the retrograde process of a reduction of brain to the condition of albumen. When the clock has gone down it is reduced to the first stage of its organic existence (viz., as it came from the artisan), which, as I have observed, may be said to be analogous to albumen in the organic world.

A difficulty may here, at first sight, suggest itself to some minds, but it is one which is more apparent than real. To those unaccustomed to reflecting on such subjects it may, on first thought, appear a paradox to say that a clock while indicating time or a brain while thinking is *dying*, when in reality *that* appears to be the time when they are most actively manifesting life. True—but what *kind* of life? The *animal* life of a brain, and *not* the *organic* life of a tree. *While* the brain is thinking it is undergoing disorganization and decomposition. *While* the brain is decomposing and disorganizing it is dying; *when* it is decomposed and disorganized it is *dead*. It has lost the organization which raised it from the condition of albumen to that of brain, and is now in a condition analogous to that of a clock that has run down.

Nay more, when that functional capacity of the brain had *once* been exhausted by a *single* exercise of thought, that brain would be

for ever as dead as a clock which had gone down, were it not that the *winder* is ever at hand to raise the weights of the brain. The nutritive process is ever present for the repair of the brain, after its exhaustion by thought, and is the *winder* for its restoration to its former condition.

The organizing atom or microzyme (name it as you please), which raises albumen to the organized residual product, known as cerebral matter, is ever at hand in the brain to carry on nutritive repair.

Were it not that the so-called "nutritive process"—namely, *organic* life—is ever in action to restore the exhausted brain, and prevent its weights going down, that brain, after a *single* exhaustion by thought, would be as dead as a clock that had gone down. The so-called nutritive process is that process whereby matter is "elevated in the scale of organization." No such provision as nutritive repair has been made in the clock to wind up the weights of *its* brain; consequently, when the clock *once* goes down, *its brain* is *dead*, and, in the absence of a winder, is permanently dead. The clock that has gone down, however, is not without organization; it has the organization conferred on it by the clockmaker; the organization of the *first* stage of its organic life—in a word, the organization of *albumen*, not of *brain*.

Thus far then may be recognized two errors in Dr. Watters' analogy: namely, 1st, He ignores the "adjustment," "arrangement of parts," or "mechanism," of the clock as constituting any part of its, so to speak, *life;* and 2ndly, he contrasts derisively *the indicating of time* with the phenomena of *organic* or constructive life, between which no analogy exists.

We are now in a position to analyse the true import of those mysterious "*logical* antecedents" and "logical consequents to *all* the various motions of the clock." Here I, too, might, perhaps, indulge in satire were I in the mood; but I am not, and will confine myself to argument. I would rather convince than affront Dr. Watters. What, then, is in reality the *first* and indispensable "*logical* antecedent to *all* the various motions of the clock?" It *is not* the action of gravity. There *must* have been "a *logical* antecedent" *preceding* the action of gravity, which latter can only be *second* in the consecutive links in causation. The *first* antecedent in the consecutive links of causation, whereby the descent of the weights of a clock is effected, is the withdrawal or "*transit*" from those weights of the force—be it what it may—which *elevated* those

weights to their upward position; and such force is a force opposed to or antagonistic of gravity. And although it is true as it is derisive to say "there is no time indicating agency radiating from the weights," it is *equally true* to say that *some* force antagonistic to gravity must have *left* or been *withdrawn* from those weights *before* they can give the slightest aid in the indication of time.

We are told "that the gravity or 'downward' tendency of the weights is the *logical* antecedent to *all* the various motions of the clock." Now what is this "downward tendency?" The weights could have no downward tendency unless they occupied an upward position. How was that *upward* position attained? It must have been by some force acting in opposition to gravity. It may have been heat, or muscular, or other force, but be it what it may, it was a force opposed to or antagonistic of gravity.

Now, *while* this force—be it what it may—was raising the weights, there was an *upward tendency* which prevailed over the downward tendency. So long as the force which "*elevated*" the weights to their upward position *is acting* on those weights, gravity is powerless to cause their descent. Consequently such force must necessarily be *withdrawn* from the weights before those weights can begin to descend. Were the winder of the clock still to keep pressure on the key when the weights had attained their utmost elevation, there would be no descent of the weights, notwithstanding the action of gravity.

Here, then, is a "*logical* antecedent" *preceding* Dr. Watters' "*logical* antecedent to *all* the various motions of the clock"—namely, the *withdrawal* from the weights of the force antagonistic of gravity which *elevated* those weights to their upward position. When this force—viz., the winder's hand—is *withdrawn* from the weights, *then*, and not till then, they begin to descend. Now observe *synchronously* or *simultaneously* with the descent of the weights the clock indicates time. It is to *this* point—namely, *the withdrawal of the winder's hand*, and *consequent* descent of the weights—*simultaneously* with which descent the clock indicates time—I would ask Dr. Watters' special attention, because it is in relation to *it*—so far as I can collect from his criticisms—that Dr. Watters appears to consider the great distinction between his views and mine is to be recognized. The difficulty with me is to recognize any agreement.

For the purpose of illustrating what I mean to convey when I state that the indispensable "logical antecedent to all the various

motions of the clock" is the *withdrawal* from the weights of that force, muscular or other, which raised them in opposition to gravity, I shall venture on a very imperfect analogy myself. I do so, not from any undue weight I attach to a fact which to me seems too obvious to require illustration, but because I am anxious to convince Dr. Watters and others that he has overlooked an important physiological law upon which I dwelt at considerable length in my publication of 1848. The law I refer to is this—there is an indispensable necessity for the action of "a specific stimulus" before our various organic tissues—brain, muscle, &c.—can normally commence the discharge of their respective physiological functions. Thus the cerebral cells require a stimulus from the sensorium before thought can commence. The retina requires the stimulus of light, muscle that of nerve impulses, &c.—all require the operation of some specific stimulus specially adapted for calling their functional action into operation. The analogy I venture to submit, though in itself very defective, will be sufficient to illustrate my meaning. It is this—if gunpowder, placed in the centre of a sphere of confined air, be exploded by the application of an electric spark, two phenomena result *simultaneously*—namely, expansion in one direction, and condensation in another—expansion of the gunpowder in the centre, and condensation of the air at the periphery. These two results occur simultaneously, but the *indispensable* "logical antecedent" of both is the application of the electric spark.

Let us now suppose the sphere of confined air with the gunpowder in its centre, *before* the application of the spark, to be analogically represented by a *cerebral cell* in its quiescent condition—namely, *before* thought has commenced, before that cell has received its first stimulus from the outer world through the sensorium, and consequently *before* that cell has developed an *idea*. Let us further suppose (and this is the weak point in my analogy, but it will serve for illustration), let us, I say, further suppose that the *condensation* of the air (one of the simultaneous results of the application of the spark) to be analogous to the *idea* developed by the cerebral cell, when that cell has been acted on by its specific stimulus (viz., by a sensation from the sensorium), and my analogy, such as it is, is completed.

When a cerebral cell receives its specific stimulus from the sensorium, the balanced equilibrium which hitherto obtained between the forces in that cell, and which maintained its components in a state of rest—that balance is *now* disturbed, molecular motion is

now set up, disintegration commences, and *simultaneously* therewith that cerebral cell developes an *idea*. This may, in a loose way, be said to be analogous to the explosion of the gunpowder, which simultaneously causes a *condensation* of the air. But the point I wish to impress on Dr. Watters is this—that the *indispensable* logical antecedent to *all* motion in both was the application of the "specific stimulus" or spark—in a word—*in both the equilibrium must be disturbed.*

Or, in the same language of analogy, the sphere of air with gunpowder in its centre may, *before* the latter is exploded, be said to represent a clock with raised up weights, which weights are still restrained from descending by pressure of the winder's hand. The balanced equilibrium between the opposing forces—gravity and the raising force—is still maintained, and consequently no motion of the weights can take place. The *withdrawal* of the hand is the application of the spark. All then becomes active, and the clock indicates time, because then *the equilibrium is disturbed.*

I shall now re-introduce Dr. Watters' analogy, for the purpose of pointing out in what way it appears to me that each statement in that analogy is erroneous. The analogy is as follows:—

"I would say that gravity or 'downward' tendency of the weights is the *logical antecedent* to *all* the various motions of the clock—that the motions of the clock are logically *consequent* upon the descent of the weights, and that the *speciality* of the motions of the clock, indicated in its keeping time, is due to the adjustment, to the arrangement of parts, to mechanism. And, as there is no time indicating agency 'radiating' from the weights, so I would say that decay or disintegration, or a 'downward' process,[a] is the logical antecedent of life, with which all the various 'vital' motions or processes are logically *consequent*—the speciality being determined by form or organization. Is there any ambiguity in that? Can any one misunderstand it, or confound it with any other 'thought' not identically the same?"

I have undertaken to make an effort to *convince* Dr. Watters that every statement in the foregoing paragraph is an error. To make this apparent I shall divide the whole paragraph into its individual statements, and place in apposition to each my own view of the same topic.

[a] The word "*is*," as above printed, in the original of Dr. Watters is "*in*," but conceiving it must be an error of the printer, I have taken the liberty of substituting "is" for "in" in quoting Dr. Watters.

DR. WATTERS.	DR. FREKE.
1. Gravity or "downward" tendency of the weights is the logical antecedent to *all* the various motions of the clock.	1. Gravity or downward tendency of the weights is *not* the logical antecedent to *all* the motions of the clock, inasmuch as the motions caused by gravity must be *preceded* by the motion or withdrawal of the hand or other force which raised the weights.
2. The motions of the clock are logically *consequent* upon the descent of the weights.	2. The motions of the clock are *not* logically *consequent* upon the descent of the weights, but *synchronous* or *simultaneous* therewith.
3. The speciality of the motions of the clock, indicated in its keeping time, is due to the adjustment, to the arrangement of parts, to the mechanism.	3. The speciality of the motions of the clock, indicated in its keeping time, is *not* due to the adjustment of parts. It is due to the adjustment of parts *plus* the withdrawal from the weights of the force by which they were raised.
4. And, as there is no time indicating agency "radiating" from the weights, so I would say decay or disintegration, or a "downward" process, is the logical *antecedent* of life, with which *all* the various "vital" motions or processes are logically *consequent*—the speciality being determined by form or organization.	4. And as there *is* an agency or force which must be withdrawn from the weights before there can be any indication of time, so I would say decay or disintegration, or a "downward" process, is *not* the logical antecedent of life, with which *all* the various "vital" motions or processes are logically consequent.
5. Is there any ambiguity in that? Can any one misunderstand it, or confound it with any thought not identically the same?	5. There is no ambiguity or, in my opinion, philosophy in that. No one can misunderstand it, or, in my opinion, confound it with any thought coinciding with "*the reality of things.*"

I shall conclude the present paper with a few words of explanation of my motive for employing the word "*radiating*," as applied to the force which elevates matter in the scale of organization. It was exclusively in relation to the phenomena of *organic* life that I used the term *radiates*, and yet it is in relation to the phenomena of *animal* life that Dr. Watters derisively applies it.

It was while matter was in the act of being *elevated* in the scale of organization that I applied the term radiates; it is while the weights of the clock are *descending* that Dr. Watters in derision applies it.

But what is there so derisive in the term *radiates?* The force, gravity, which causes the descent of the weight radiates; why, then, such "peculiar phraseology" the suggestion that the force that raised them may also have radiated? Had the force which raised those weights been heat, instead of muscular force, would it not, on ceasing to act on those weights, have *radiated?* And would it not have been competent, had appropriate arrangements been made, to raise equal weights to a like elevation? Does not heat *radiate?* Does not light *radiate?* Does not gravity *radiate?* Why, then, should it be regarded as such "peculiar phraseology" to suggest that the force which elevates matter in the scale of organization also may *radiate?*

But let us turn from the contemplation of the mechanical elevation of the weights of a clock to the *vital* elevation of matter in the scale of organization. Let us contemplate the elevation of albumen to the organic condition of brain. Is it not a *fact* that the cerebral cells are *spherical* in form? Is there nothing suggestive of a *radiating* force here? Is it not a *fact* that the formation of a "nucleated cell" *precedes* the completion of several other organic tissues? Is there no hint of a *radiating* force here?

It was for the express purpose of pointing out what I conceived to be the error entertained by physiologists as to the nature of the so-called "nucleated cell," that I contemplated the action of the great forces in nature, and observed that their action was *radiating.*

When I published (1848), the opinion entertained I believe almost universally by physiologists was, that the "nucleated cell" was the *ne plus ultra* in biological analysis. I ventured to maintain the contrary, and contended that such cell was but *a stage* of a progressive process, and was preceded and formed through the agency of a *radiating* force.

There are, I believe, at the present moment, but few physiologists who do not consider the opinion previously entertained as to the nature of the "nucleated cell" to be erroneous. How far I may have aided in dispelling that error may possibly be decided by posterity—in the present age I believe it will not.

(To be continued.)

ART. XVII.—*Report on One Hundred Extractions of Cataract by the Peripheral Linear Section.* By HENRY WILSON, F.R.C.S ; M.R.I.A.; Assistant Surgeon to St. Mark's Ophthalmic Hospital; Examiner on Ophthalmic Surgery in the University of Dublin; Surgeon City of Dublin Artillery; &c., &c.

IN the number of this Journal for May, 1870, I communicated a brief historical notice on extraction of cataract, and attempted to describe a method of removing the cataract which was devised and practised by the late lamented Professor v. Gräfe. Since writing that essay I, together with very many others, have had to mourn the loss of that gifted man; by his death on the 20th July, 1870, ophthalmic science has sustained the heaviest loss and one which seems to me irreparable in the present day. Albrecht v. Gräfe was beloved and almost reverenced by all who knew him, and honoured and admired by hundreds who had not the advantage of his personal acquaintance; he compressed into a very brief space of time one of the most brilliant careers on record, and though young in years, yet ripe in honours and glorious achievements, he passed away in the zenith of his fame, at the early age of forty-two. Though dead, yet shall he live in the discoveries and works he has left behind him.

Having in the essay above alluded to endeavoured to draw attention to v. Gräfe's peripheral linear method of extracting, I now propose to give a short summary of one hundred operations by that method. I regret that time will not permit me at present to do more than give brief notes. I hope, however, on some future occasion to give a more detailed account of a second series of one hundred extractions. These notes, crude as they may be, furnish, however, information respecting the merits or demerits of v. Gräfe's modified linear extraction, for I think I may claim that one hundred cases of any one operation constitute a sufficient number to base calculations upon; they possess to my mind also the additional advantage of exhibiting the statistics of extraction in Ireland, which so far as I am aware, has never been previously attempted. These operations were all performed by me in St. Mark's Ophthalmic Hospital; the cases are not selected ones, but are taken from my note-book consecutively as they occurred.

These operations were undertaken on 78 individuals, 47 males and 31 females; of these 17 persons had both eyes operated on at

different periods, varying from one week to two years between the two operations; 5 persons had both eyes operated on at the one sitting. Of the hundred operations 52 were on the left side and 48 on the right. The average *age* at which the operation was undertaken was 58.

VARIETY OF CATARACT.

Only 35 individuals presented uncomplicated ordinary mature cataract in both eyes; 13 had uncomplicated mature cataract in one, and immature cataract in the other eye; amongst the former I have included one female who presented in one eye Morgagnian cataract, and three persons with capsulo-lenticular cataract. The remaining eyes presented some complication.

Complications.—Were I desirous of choosing unpromising cases to test this operation on I could scarcely have selected more appropriate ones than a large number of these 78 persons proved to be; 11 individuals had had one eye unsuccessfully operated on prior to their coming under my care; 8, some of them included in the foregoing, had one eye collapsed or atrophied from injury, operation, or disease; 4 had suffered from choroido-iritis which had resulted in discoloured iris and closed pupil with complete extinction of vision; of these one had the right eye collapsed; 6 presented pupillary adhesions; one person presented a traumatic floating cataract with the pupil dislocated downwards and outwards; another had the cataract dislocated inwards and the pupil adherent externally, and there was in addition pterygium; another individual presented also on the operated eye pterygium, and on the non-operated eye choroido-retinal atrophy; 4 persons with cataract on one side presented diseased fundus on the other side without opacity of lens; 7 individuals were affected with granular conjunctivitis at the time of operation; one woman who had been unsuccessfully operated on elsewhere on one eye presented on the other side, the upper half of the cornea opaque; one man was so crippled with rheumatism that he had to be operated on in bed in his ward; another man had partial paralysis of the lower extremities; one woman had prolapse of the uterus; another woman, aged 75, was affected with most distressing asthma, and had lost her husband and several members of her family shortly before admission into the hospital; one man was affected with enormous glandular tumour of neck and adherent pupils; one had gonorrhœa; one man was a broken down drunkard

and saturated with tobacco; one man, aged 71, spoke Irish only; one woman who had had her right eye unsuccessfully operated on some years previously was a lunatic. Some of these latter cases not having presented any ocular complication are included in the 48 uncomplicated cataracts above alluded to.

Chloroform was administered to only one of these 78 persons; in this case I commenced the operation without its aid, but the man became so unruly and the muscular spasm so great that the already very prominent eyebrows became so contracted over the small sunken globe as to render access to the eye almost impossible, and the completion of the section most difficult. After chloroforming him both cataracts were removed with a most favourable result on one side and three-quarter result on the other.

OPERATION.

I have but little to add to what I have already written respecting the operative procedure. I would again caution against making the counter-punctuation too far backwards, and making the wound too long, both of which faulty steps are generally punished by prolapse of vitreous humour. A straight iris forceps will be found best for making the iridectomy.

The complications and accidents during the operation were of the most diverse kinds, some of them avoidable others unavoidable. The conjunctiva was torn by the fixation forceps in six instances.

The section was almost altogether in the transparent cornea in one instance. The section in another instance was so far back in the sclerotic that it passed through the ciliary attachment of the iris, and the latter instead of bulging into the wound lay up against the back of the cornea, and allowed the vitreous to escape. In five cases the counter-punctuation was made too far backwards; in three the point of the knife was withdrawn and inserted at the desired position.

In two cases the counter-punctuation was too much in the transparent cornea. In one instance the section was calculated too small, and the lens being very large the wound was enlarged with the scissors.

In one case there being no anterior chamber very great difficulty was experienced in passing the knife between the cornea and iris.

In three instances the iridectomy was incomplete or imperfect; in one case owing to a bad scissors, and in the other two owing to

great loss of vitreous following section. In one of these the section was made too large.

Pupillary adhesions had to be divided in nine instances. This was done generally by passing the cutting edge of the cystotome round between the iris and lens. In one instance after the lens was hooked out it remained still attached by membranous bands which had to be divided with the scissors.

Loss of vitreous humour occurred in 29 cases; once before completion of section; 3 times immediately after completion of section; in one of these the eye was not being even touched at the moment when I observed the fluid vitreous begin to ooze out; 8 times after the iridectomy; 6 times on lacerating the capsule, and 11 times after the removal of the lens. The cataract was dislocated during the act of opening the capsule in 3 instances; it fell down into the bottom of the fluid vitreous in 2 cases.

The cataract was removed by means of the hook in 5 instances, by means of the spoon in 8, and with the cystotome in 2. In 1 case the cataract was removed in its capsule, and in 7 instances the capsule was removed by means of forceps or hook. In 3 of these the capsule was opaque.

In the case of Morgagnian cataract immediately on opening the capsule there was a gush of the Morgagnian fluid, and the small hard smooth and almost round nucleus was easily pressed out.

In one instance of normal extraction in removing a clot of blood from the angle of the wound, I unwittingly seized together with the coagulum a piece of iris and tore the ciliary attachment of the latter which gave rise to hæmorrhage.

In 14 cases I left some lens remains behind. In one instance where I had made the section too much in the transparent cornea the vitreous humour bulged up into the wound after the removal of the lens keeping the edges of the wound apart. I snipped off the projecting humour and bandaged down the eye. In the remainder of the cases the course of the operation was normal.

HEALING PROCESS.

The wound as a rule heals readily and quickly, and has united generally by the second or at the farthest the third day. In only 7 cases did it not so unite. One remained patent for 13 days and the other for 10 days, and both terminated in cystoid cicatrix.

Two of these 7 cases suppurated, the wound not closing at all, the others closed after a few days and did well. *Iritis* is the most

frequent amongst the unfavourable consequences of the operation, it occurred in 27 of these 100 operations and caused generally more or less occlusion or adhesion of the pupil. Iritis or choroido-iritis set in from 8 hours to 5 days after the operation, and in a few instances exposure of the eye to cold was the exciting cause; one patient whose vision was quite good 4 days after the operation, got iritis from going surreptitiously into the court yard of the hospital on a cold wet day. This iritis is sometimes accompanied by chemosis and pus in the anterior chamber; this occurred in 4 instances; the inflammation is generally insidious, occurring with but little or no pain. In a few instances however there was considerable pain and violent inflammatory symptoms, such as œdema of the lids, chemosis of the conjunctiva, great vascular injection, effusion through the pupil and complete discolouration of the iris with turbidity of the aqueous humour; occasionally the cornea was implicated and became a little hazy, but as a rule it was not much affected in these operations. Lens fragments left after the operation are very likely to cause iritis and neoplastic membrane.

Five cases healed with complete occlusion of the pupil and 16 with partial occlusion. Several healed well with lens fragments in anterior chamber in or behind pupil, or with partial opacity of capsule. Hæmorrhage occurred in 5 cases during the healing process after the operation, it occurred only to a slight extent into the anterior chamber, and produced no ill results except in one case about to be mentioned, where it occurred shortly after the operation and was profuse; in one case it occurred five days after the operation and nearly filled the anterior chamber.

The following are the cases in which *suppuration* or *disorganization* followed the operation:—

1. In the case where the prolapsing vitreous was snipped off alluded to above, the man got violent pain two hours after the operation, and when I saw him immediately afterwards I found the eye bleeding, the wound gaping open, and filled apparently by the vitreous humour and blood; the hæmorrhage was considerable and lasted for some hours, notwithstanding the application of ice and other treatment, and the pain was agonizing; the lids swelled that evening, and the eye finally suppurated. This man had had his other cataract most successfully extracted by me twelve days previously.

2. A feeble decrepid old woman with heart disease and long standing cataract, in whom I made the section too small and had

to enlarge it with the scissors, and had great difficulty in removing the large lens with the scoop, got vomiting on the evening of the operation, and two days afterwards the cornea became grey, the lids œdematous, the extremities cold, and diarrhœa set in; on the fourth day pus began to form in the anterior chamber, the cornea necrosed, the wound opened and the globe gradually suppurated.

3. A man who had his right cataract removed by the old flap extraction and lost the eye by violent suppuration, had his left removed by me by the linear section 6 weeks subsequently; some lens fragments were accidentally left behind. Seven days afterwards some hæmorrhage took place into the anterior chamber; in 10 days the iris became discoloured, the pupil was blocked up, and the eye became somewhat soft and small, and he left the hospital without any vision.

4. An old asthmatic woman, aged seventy-five, who had sustained the loss of several of her family shortly before coming into hospital, was affected with a dense hard chalky cataract of several years standing on the operated side, and ordinary mature cataract in the other. Just as I was about to make the counter-punctuation she put up her hand and pushed mine so that the counter-punctuation fell out too far backwards, the fluid vitreous escaped in large quantity, and the iridectomy was incomplete in consequence; the lens was removed with scoop, and the eyeball collapsed almost completely immediately afterwards. She tore the bandage off and was very restless during the night, and when I visited her next morning the wound was widely gaping, the globe greatly injected, the cornea hazy, the lids swollen, and violent suppurative inflammation was soon established, which totally destroyed this eye. I subsequently chloroformed this patient and removed her left cataract with the most perfect and satisfactory result. This latter operation is not included in the present series.

5. A woman, aged seventy-one, whose left eye had been unsuccessfully operated on elsewhere, and whose right cornea presented at its upper half an opaque condition, and who was affected with granular conjunctivitis had the inferior peripheral linear section done on the right eye. There was some loss of vitreous; iritis supervened, with effusion of lymph, and the eye became gradually soft and partially collapsed.

Effects of the operation on the Eyeball.—As just shown of the 100 eyes operated on, 3 perished totally from panophthalmitis with very violent symptoms, and presented an unsightly appearance in their

collapsed condition; of these 1 was preceded if not caused by profuse bleeding—the blood flowing in considerable quantity out of the eye—an occurrence which is so exceptional that I have only witnessed it in this one instance; 2 other eyes were also lost but from a milder form of suppurative choroiditis which did not implicate the cornea to the same extent, and which did not result in the same unsightly collapsed condition as the other three; 2 eyes recovered with a cystoid cicatrix at site of wound, 5 recovered with complete occlusion, and 16 with partial occlusion of the pupil, and 10 presented capsular opacities. As the iridectomy is concealed by the upper lid as also the cystoid cicatrix and the occlusion of the pupil does not form a disfigurement, I may say that 95 of these eyes recovered from the operation with almost normal appearance.

SECONDARY OPERATIONS

Were necessary in 30 instances; these consisted in iridectomy where the pupil was dragged up and healed into the wound which occurred 3 times, and in the use of needles; where the new membrane is extensive and dense I generally employ two needles. Of the 80 perfect recoveries mentioned further on 21 required a secondary operation at some time or other—even as long as a year after the original operation.

GENERAL RESULTS.

Of the 11 persons who had been unsuccessfully operated by other methods prior to my operating on their remaining eye by v. Gräfe's method, 6 recovered vision perfectly, 1 got iritis and occlusion of pupil with improved vision which might have been rendered almost perfect by a secondary operation; 1 got some iritis, but being almost a lunatic she would not allow her eye to be looked at or treated, and insisted on leaving the hospital a few days after the operation; I believe she recovered some useful sight; in another of these 11 cases the eye recovered perfectly from the operation, but vision remained stationary, the retina being detached; 1 eye was lost from violent panophthalmitis, and 1 from gradual suppurative choroiditis.

Amongst the most satisfactory cases were the 5 persons who had both their eyes operated on at the same time; 4 of those persons regained perfect vision in each eye, and spent from 7 to 18 days only in the hospital; the fifth was the man I had to chloroform after I had made the section in one eye, and this eye did not make

a very good recovery owing to his unsteadiness while I was making the section. He recovered useful sight in it and perfect vision in the other.

Amongst the cases I submitted to this operation were 4 cases of old choroido-iritis with green or yellowish green iris, adherent pupil and opaque lens; in one instance in which there was commencing choroiditis with slight detachment of the retina in the other eye, I undertook the operation on the blind eye chiefly as a prophylactic measure; the operation did well, but there was of course no vision; she expressed herself as improved in the other eye; in 2 of these cases the lens had become chalky, and as the poor creatures had come from the extreme remote districts of Ireland, one from the North and the other from the South, and each begged to be operated on, I performed the operation, from which both recovered perfectly, but of course without any vision, the fundus in each being totally disorganized. The fourth case was that of a man whose right eye was collapsed and who possessed faint perception of light; he was so far improved that he could see though not to recognize objects; his pupil became occluded, and I am still hopeful of giving some useful sight.

One man whose right cataract I had most successfully operated on, had his left extracted two months subsequently, got iritis with occlusion of pupil, and vision remained as it was before the operation, but might have been greatly improved by a secondary operation. In the case of pterygium and luxated lens with partially adherent pupil, the eye recovered well from the operation, but the retina was found to be detached and vision was but slightly benefited.

RESULTING VISION.

Perfect vision, - - - -	80
Useful vision, - - - -	7
Improved but not useful, - - -	2
Same as before operation, - - -	6
Lost, - - - - -	5

I have made use of the term perfect vision in the above summary more for brevity's sake than to denote that the 80 eyes possessed accurately and optically perfect vision—the majority had never learned to read—but they could distinguish small objects at ordinary distances and go about their usual occupation with perfect ease.

I am often consulted as to the advisability of operating on one fully formed cataract where the vision of the second eye is still good but in which commencing cataract exists. My practice is to operate on the mature cataract at once, with a view of restoring sight and saving the patient from that most distressing period during which the cataract is "ripening;" if avoidable I do not see why patients should be afflicted with the sad and disheartening process of going blind. Moreover poor persons who are sent up to the hospital from long distances if returned to wait until the second eye is also blind may never have a second chance of being sent back to the hospital, and again over ripe cataracts and such as are of long standing are not by any means the best for operation. Several of the above cases were operated on on this principle, and some of them have had their second eye already operated on, and I expect still to operate on the second eye of others.

Where the patient is affected with cataract in both eyes either mature or approaching maturity and does not object, I prefer operating on both eyes at the same sitting. So far as my cases have gone the success of the double operation is great, the patient's suffering is probably lessened, and there is also great saving in time.

As already stated only one of the above patients was chloroformed, and that may to a great extent account for the numerous accidents during the operation; it is very difficult to execute the various steps of this operation with the necessary nicety, &c., in an unsteady patient, it has happened to me that the patient rolled his head about or even caught my hand during the operation; the loss of vitreous humour is frequently caused by the involuntary struggle and muscular spasm of the eyeball during fixation, what between the spasm and the natural tendency of the eye to roll upwards and the pressure made by the fixation forceps in drawing it downwards, it is easily imagined how the hyaloid membrane may be ruptured and the vitreous escape. Some of the accidents and ill results were, no doubt, attributable to my own faulty manipulation or errors of judgment, and therefore avoidable, and I have no doubt but that these untoward events would have been greatly lessened had chloroform been used. The operation is rendered much more facile and much freer from accidents by the administration of chloroform, the only drawback to which is the subsequent sickness of stomach; if, however, all lens fragments can be removed and the eye bandaged before this comes on I do not think it can affect the

eye much. The beneficial effects of chloroform administration was especially marked in the old woman whose right eye was lost by suppuration, and whose left was operated on under chloroform, and the vision perfectly restored.

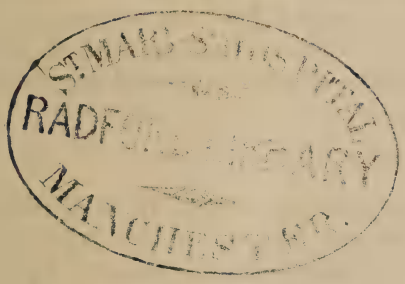
I think I have now shown good cause for the belief which I expressed last year in the safety and utility of this operation. I then stated my belief "that in this new operation we have a much safer and more speedy means of curing cataract than in any method hitherto practised." I think 80 per cent. of perfect recoveries is, considering the material I had to work upon, a very satisfactory result. In at least 4 of these 100 cases I could not possibly expect recovery; had all my cases been uncomplicated cataract my per centage of recoveries with perfect vision must have been about 88 at least. Summing up my results I find that 80 per cent. were perfect, and 15 per cent. imperfect results, and of these 15 some are capable by secondary operations of being converted into perfect results; 5 per cent. were lost.

ART. XVIII.—*Ophthalmic Notes.* By H. ROSBOROUGH SWANZY, M.B.; Ophthalmic Surgeon to the Adelaide Hospital; late Assistant at the late Prof. von Graefe's Ophthalmic Hospital, Berlin.

I.—*A peculiar Form of Retinitis Pigmentosa in connexion with Inherited Syphilis.*

I HAD the following case for some months under observation at the Eye Dispensary of the Adelaide Hospital.

Samuel C., aged eleven years and six months, came on the 27th June, 1870, complaining of imperfect vision of the right eye. It had always been weak, but had become much more so of late. It was affected with an external strabismus of about two lines, and with nystagmus. It was somewhat smaller than the left eye. The iris presented a dull, diffused appearance, its fibrillæ not being distinctly marked as in the normal eye. The pupil was perfectly dilatable with atropine. The patient said the sight of the affected eye became much worse in the dusk of the evening. By good light he read with this eye small words of No. 8 Snellen at six inches, and CC at fifteen feet, glasses making no improvement. The eccentric field of vision was unimpaired, for even with a very imperfect illumination he counted fingers in all directions. The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Dr. C. E. FitzGerald, ad nat. pinx. et lith. Forster & Co.

Mr. SWANZY. A PECULIAR FORM OF RETINITIS PIGMENTOSA, IN CONNEXION WITH INHERITED SYPHILIS.

[Inverted image enlarged seven diameters.]

left eye was slightly myopic, but its vision was nearly normal, and externally it appeared quite healthy.

The ophthalmoscope revealed those changes in the fundus of the right eye, which are so admirably represented in the accompanying drawing by Dr. C. E. Fitzgerald; and I cannot lose this opportunity of returning him my sincere thanks for the great trouble he has been at, not only in making the original drawing from life with the greatest accuracy, but also in executing with such care the various stones for the lithograph.

The optic disc was of a dirty greyish colour, and its margin indistinctly defined. The veins were normal, but the calibre of the arteries was somewhat diminished. In the retina, towards its periphery, depositions of pigment in the form of numberless small dots and streaks were seen. During six months, while the case was under observation, although these specks and streaks became more numerous, and extended somewhat more towards the centre, yet they had no tendency to become confluent, or to form themselves into stellate or bone-corpuscle shapes. They were not deposited along the course of the arteries; on the contrary, they appeared to avoid the vessels, and to occupy by preference the spaces lying between these. At one place only (above and to the left in the drawing) was I able to discover one or two specks which lay over a vessel. To the inside of the optic disc (in the inverted image), and again in the lower and outer quadrant towards the periphery, patches of choroidal changes were visible. These changes consisted chiefly in the disappearance of the pigment of the epithelial layer, but in the peripheral patch a commencing atrophy of the choroid seemed perhaps to exist. In the fundus of the left eye two patches of choroidal changes similar to those in the right were present, but only two or three small dots of pigment were observable in the retina, and the optic disc and vessels were normal.

The patient was of a sallow earthy complexion, with a flabby skin. One of the upper incisors was very characteristically notched, the other not so; the canine teeth were peg-top shaped, and the lower incisors were vertically notched. The head was large. The patient had been born healthy, but when a fortnight old had suffered from ulcerations about the anus, &c. About a year ago he had had a swollen knee, unassociated with any trauma; the swelling had been painful, but not red or inflamed-looking. This is the eldest of four children, of whom two

had been still-born, and one, besides our patient, is living. The last conception had been followed by a miscarriage. The other living child is the youngest, not reckoning the miscarriage, and, with the exception of being somewhat deaf, it is and has been a healthy child. The father and mother had not been blood relations. The latter died a short time ago. Five or six years before his marriage the father had suffered from a gonorrhœa, but he could not say whether he had had a chancre at the same time or not. The medical man, however, to whom he had applied gave him pills, which made his mouth sore. He said he might have had a chancre between that time and his marriage, but he could not distinctly remember. Six or seven years ago he had ulcerations on the calves of the legs, which have frequently reappeared since, and large scars now mark the place where they were situated. His wife had told him that the dead children had been both "diseased."

The patient was so irregular in his attendance at the dispensary that a steady course of treatment was impossible. I prescribed the iodide of potash in the first instance, and this was repeated occasionally, with apparently good effects, for, at his last visit (5th January) he said he could see much better in the dusk of the evening, and he could read short words of Snellen No. $5\frac{1}{2}$ at 6-8 inches with his right eye, and this, although the pathological changes had distinctly advanced. In the left eye the patches of choroidal changes had progressed somewhat towards a more decided atrophy, but the vision remained as good as at the first visit.

With the exception of one case of Mr. Hutchinson's, to which I shall draw attention further on, I have been unable to find any similar to the above on record. It would be inadmissible to form decided conclusion from an isolated clinical observation, and I merely desire to indicate those points in the case which appear of greatest interest, for the purpose of making it more available to the general readers of the Journal, and in the hope of inducing those specially interested in diseases of the eye to record their experiences and opinions with regard to those points.

In the first place, with regard to the ophthalmoscopic appearances. These differed from the usual picture of retinitis pigmentosa (*vide* Liebreich's *Atlas of Ophthalmoscopy*, Plate VI. Fig. 1) in *the shape of the pigmentary depositions, which were in the form of little dots and strokes, but not as stars or Haversian systems*, and, what would still more incline me to regard the disease as distinct, *these deposits did not occur along the course of the vessels, but in the spaces*

between them, a narrow clear interval being commonly left at either side of the vessels, as is well seen in the drawing. In other respects the fundus of the right eye was not materially different from one affected with ordinary retinitis pigmentosa. Again, with regard to the functional disturbances, a contraction of the eccentric field of vision, as the reader is aware, is, I may say, a constant symptom of the typical retinitis pigmentosa; so that, although these patients may be able to read moderately small type, they will be unable to go alone in the street (a state of vision which may be imitated by looking through a small roll of paper or other narrow cylinder). In fact, by the gradual contraction of the field of vision from its periphery towards its centre (the macula lutea), it is, that these persons ultimately become blind. In our case *the field of vision was perfectly normal.*

The evidence of inherited syphilis in this case will appear insufficient to some, and I confess the proofs might be more conclusive. I give, however, below, Mr. Hutchinson's own authority for regarding them as adequate.

Are the changes in the fundus oculi in this case to be regarded as depending upon and caused by the inherited disease—in fact, as a retinitis pigmentosa syphilitica? Or, are they to be regarded as an unusual form of pigmentary degeneration of the retina, occurring by chance in a person affected with inherited syphilis? Further experience can alone decide these questions.

Having failed to find any parallel case in the literature, I communicated the above to Mr. Hutchinson, with a drawing of the teeth, begging him to acquaint me with his opinion, &c. He kindly sent me the following reply, which I publish with his permission:—

"4, Finsbury Circus, E.C.
Jan. 14, 1871.

"My Dear Sir,—I am very sorry to have been so long in replying to your letter. I am very much interested in the subject, and thank you warmly for sending me the case.

"In the first place, from the history and state of the teeth, I should feel no hesitation in believing your patient the subject of inherited syphilis, and that whatever is present in his eyes is due to that cause.

"I have long taught that conditions very similar to those found in what is called retinitis pigmentosa may occur in consequence of inherited taints. At page 324 of the Ophth. Reports you will find one case, but since then I have seen several much more marked, and more conclusive, and I have just had a drawing made of one.

"The syphilitic cases resemble the other forms of retinitis pigmentosa in the existence of hæmeralopia. The arrangement of the pigment in a zone at the periphery. The presence of pigment in the retina. The arrangement of the pigment in stars, &c. The waxy atrophy of the disc, and extreme shrinking of retinal vessels.

"They differ, I think, in the following points:—The rapidity with which the changes advance, and the early age at which blindness is brought about. They are seldom symmetrical, or, at any rate, the two eyes rarely suffer in like degree. The pigment is often in blots or round specks, as well as in lines and stars. There is often evidence of disease of the choroid as well. The pigment is more often irregular in its arrangement than in the more characteristic cases not due to syphilis.

"Still I have no doubt that inherited syphilis is not infrequently the cause of affections which are spoken of as 'retinitis pigmentosa.'

"Believe me, &c.,

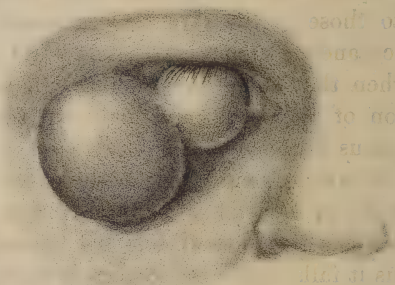
"JONATHAN HUTCHINSON."

The following is the case referred to in the foregoing. *Ophth. Hosp. Rep.*, Vol. v., p. 324:—Case of 'Choroido-retinitis' in a girl, aged seventeen.—"The central incisors most typically notched, but no history of syphilis could be obtained.

"*Ophthalmoscope.*—Each eye the same. The discs of a dirty grey white, and the vessels so small they could only be traced with difficulty beyond the margin. There are but few denuded patches of sclerotic. The pigment was accumulated in 'blots' rather than stellate patches. There were a great number of these 'blots,' and more of them near the centre than at the periphery. Many of the pigment patches were in the retina itself."

I confess I am much inclined to agree in Mr. Hutchinson's opinion, that many cases which are regarded as ordinary retinitis pigmentosa depend probably on inherited syphilis. Attention has been little directed to this point.

Mr. Macnamara, of Calcutta, in his excellent *Handbook of Diseases of the Eye*, p. 401, says:—"I am disposed to look upon the disease [retinitis pigmentosa] as a result of inherited syphilis. . . . . The only facts which, in my mind are opposed to this view of its relation to syphilis are, that I have not noticed the notched teeth, nor have I seen the affection in more than one member of a family; and lastly, the disease does not seem to be influenced in the least by any treatment with which I am acquainted."



MR. SWANZY. DERMOID OF THE CORNEA. (LIFE SIZE.)

II.—*A Case of Dermoid Tumour of the Cornea.*

It needs only reference to any text-book of ophthalmology to ascertain that the dermoid is the most common form of tumour of the cornea. Its usual seat is on the outer border of the cornea, extending over to the conjunctiva more or less; indeed, it is as often described under the head of diseases of the latter membrane as among those of the cornea. These tumours are for the most part congenital, they are rarely larger than a pea, and seldom increase in size after birth. They sometimes extend so far over on the cornea as to interfere with vision, and, whether for this reason or upon cosmetical grounds, their removal is often desirable. This little operation must be practised with great care, as the growth frequently implicates nearly the whole depth of the cornea, and then a radical extirpation would be attended with danger to the eye. Experience, however, shows that the portions of the tumours which are left behind, usually become absorbed, and then very satisfactory results are obtained. The name of dermoid was first applied to these tumours by Ryba,[a] who found that they were composed of elements similar to those of the skin. Occasionally they partake so slightly of the cutaneous structure as scarcely to deserve their name; or again, when they sometimes increase in size, they do so by the accumulation of fat tissue, and are then termed by some, lipomata, or lipomatous fibromata.

I observed the following remarkable case at the hospital of the late Prof. von Graefe, in December, 1869. It was to have been published by my late master himself, but his lamented death prevented this, and thus it falls to my lot to place it on record. For the notes of the microscopical examination I am indebted to my former colleague Herr Dr. Leber, who had prepared them for von Graefe's use. The accompanying drawing was taken from life, and is a faithful representation of the original.

Martha M., aged eight months, was admitted to hospital on account of a tumour of the cornea of the right eye. The patient was otherwise perfectly healthy. The tumour was congenital, and was remarkable for its size, being, I believe, the largest dermoid tumour of the cornea which has as yet been recorded. It consisted of two portions, an anterior one of about the size of a large cherry, and a posterior one less than half the same size. The two segments were connected by a narrow band. It appears probable that this peculiar

[a] Prager Vierteljahrsschrift, Bd. III., 1853.

shape depended on the constriction the tumour was subjected to by the eyelids *in utero*. The base of the posterior segment occupied the whole of the cornea, with the exception of a small part about a line in width at the inside, but it did not extend over on the conjunctiva in any direction. Both segments were non-translucent and were soft to the touch. They were covered with a delicate cuticle, upon which, with the aid of a loup and in strong light, a few fine pigmentless hairs could be discerned. The colour of the entire growth was that of the ordinary integument. The tumour did not impede the motions of the globe, but followed them in every direction. According to the mother's account, the anterior segment had been growing slowly, until at the time of admission it had attained double its original size. The posterior segment had remained stationary.

Prof. von Graefe removed the tumour, but it was not possible to accomplish this in the way usually recommended for these tumours, by merely shaving it off the corner, for in this instance the tumour occupied the whole depth of the cornea. It was necessary, therefore, to open the globe of the eye, and an aperture was left where the base of the tumour had been situated. Through this a considerable portion of the vitreous humour escaped; the crystalline lens, however, could not be found, and was thus shown to have been congenitally deficient.

The microscopical examination showed that the tumour consisted of an external cutislike layer, and beneath this of a thick layer of fat tissue, of which, in fact, the great mass of both segments consisted; and this tissue differed in no way from the usual subcutaneous fat. The cutislike layer was formed of a superficial epithelial covering, and of a layer of cellular tissue containing hair follicles, and very beautifully developed sudoriferous glands, the latter extending as deep as the fat tissue. There were few papillæ, and these were imperfectly developed; on the other hand, in some places, numerous and tolerably large vessels were observable. It was with difficulty that some minute rudimentary sebaceous glands were discovered in connexion with the hair follicles, but the ordinary acinous sebaceous glands were not present. The cornea, as such, was quite absent, but a thin layer of fibrous tissue which occupied an analogous position, might be supposed to take its place. It could not be ascertained whether the tumour had caused an arrest in development of the cornea, or whether the latter had been gradually destroyed by the growth of the tumour. A rudimentary

iris lay close against the pseudo-cornea behind, so that there could have been no anterior chamber. Concerning the size of the globe it was not possible to form an exact estimate, for it was at first so concealed by the tumour, and after the operation it became collapsed. It did not appear, however, to deviate much from the normal size. That the name of dermoid is properly applied to this tumour, is evident from its minute structure.

It is not necessary to try to account for the heterology of the tumour. There is nothing more wonderful in such a growth occurring on the cornea, than in an ovarian tumour which contains hair and teeth. The fact of the deficiency of the crystalline lens has, however, suggested to me a possible explanation for this case, which I may be permitted to mention. As the reader is aware, the crystalline lens, which is an epithelial structure, is developed in the fœtus by the invagination of a portion of the cuticle in a depression of the primary optic vesicle. If we suppose this invagination not to have taken place, or that instead of being folded inwards, the cuticle had been folded outwards, no lens would have been formed, and we may easily imagine how such a growth as this may thus have had its origin.

I must not omit to mention, that Ryba believed many of these dermoids to originate in an arrested development of the eyelids. In our case the eyelids were perfectly normal.

Manfredi, of Pavia, has published[a] an interesting case of dermoid of the cornea accompanied with arrested development of the globe, the latter being in a condition of microphthalmos; but it does not appear that one part of the eye was more imperfectly developed than another, or that any part was completely absent.

III.—*An After Danger of Peripheral Prolapse of the Iris.*

The dangers, immediate and remote, which are most liable to attend prolapse of the iris into a perforating wound or ulcer of the cornea, are so well known, that it is needless to dwell on them here. The three following cases, however, demonstrate a danger incident, I think, to peripheral prolapse, which appears to be less known. I give but a short note of each case.

1. R. B., aged forty, was admitted on the 4th May, 1869, on account of a purulent iritis of the right eye, attended by

[a] Lipodermoide congenito con difetto di sviluppo dell'occhio. Revista Clinica. 1869.

agonizing pain. The inflammation had commenced two days previously, and until then he had had good use of the eye. About twenty years ago the patient had received a perforating wound of the sclerotic, to the outside from the cornea, and about half a line removed from the latter. Prolapse of a portion of the iris into the wound occurred at the time. The wound healed, and the prolapsed portion of iris remained in it, leaving a coloboma to the outside of the true pupil. The cicatrix containing the prolapsed iris was but little raised over the surface of the sclerotic, and there was little or no dragging of the iris towards the cicatrix. The patient had not since then received any injury to the eye that he can remember, certainly none within the last few days. He had of late been a good deal exposed to the weather, and had had much fatigue. At the time of admission the perception of light was good, but the projection imperfect.

The patient remained five days in the house, and was then obliged to leave in consequence of his private affairs. During this time, in spite of treatment, the functions of the eye sank gradually until they had reached a minimum, and all hope of saving the vision was gone.

2. L. M., aged thirty, admitted 6th May, 1869. Eight months before admission he received a perforating wound in the right eye, to the outside of the cornea, in the corneo-sclerotic junction; a portion of iris was prolapsed into the wound, and a coloboma was left in place of the prolapse. The wound had healed without any inflammatory reaction, the iris remaining adherent in it. The cicatrix was not prominent. The iris was dragged a good deal over towards the cicatrix. Since that time the eye had given the patient no trouble, and its vision had been perfect. The day before admission, without any trauma or other immediate cause, the eye became irritated; towards evening this irritation increased and great pain set in, and early next morning the patient came to the hospital. Purulent iritis had already commenced. The process could be distinctly seen to be most intense towards the point of incarceration. The whole iris had, however, assumed a yellowish hue, and the aqueous humour was opaque. The slightest pressure on the eye gave much pain, showing that the ciliary body was inflamed. The perception of light was good. The projection was in every direction uncertain. A very doubtful prognosis was given.

All our efforts to arrest the suppurative process proved unavailing. The power of vision sank daily until it quite disappeared.

The eye became rapidly phthisical, and it was soon necessary to enucleate it in consequence of its fellow becoming sympathetically irritated.

3. F. R., aged twenty-seven, ploughman, admitted 25th November, 1869, with panophthalmitis of the right eye. Ten days before, in the forenoon, he had received a slight blow from the hairs of his horse's tail in this eye, while stooping to arrange some part of the harness. He experienced much pain during the remainder of the day, and by next morning the eye was quite blind. On examining the eye the pupil was found very much displaced upwards, in consequence of an old prolapse of the iris into an ulcer at the upper corneo-sclerotic boundary. The great distension of the globe had now caused the cicatrix to gape. In the left eye a symmetrical ulcer had been present, leaving a similar condition of the pupil and iris. It now appeared that two years previously he had suffered from an acute conjunctival blennorrhæa in both eyes, which had been the cause of these ulcers. Since then he had had good use of his eyes.

The treatment adopted in Cases 1 and 2 was actively antiphlogistic. Mercurial inunctions until the gums were affected, emetics, purging, and locally, periodical warm fomentations, the pressure bandage, and atropine.

It is remarkable that the first two cases should have been under treatment simultaneously, and should have been so similar in many points. They differ, indeed, considerably in the length of time which had elapsed since the receipt of the trauma and the occurrence of the inflammation. In one 20 years, in the other 8 months.

In Case 2 it was evident that the inflammatory process originated in the iris at the place of incarceration. In Case 1 the suppuration had advanced too far to decide this with certainty from the appearances themselves, but in the absence of any reason for so violent a process, and with the aid of Case 2, all reasonable doubt as to the prolapse being the starting point of the iritis, must be removed. With regard to Case 3, such an injury would never produce an inflammation of this kind in a healthy eye, whatever other serious consequences it might have.

It would seem then—*a*. That the presence of a peripheral prolapse of the iris renders the eye always liable to be suddenly attacked by a purulent iritis *foudroyante*; *b*. That the immediate exciting cause may be very slight or even not discoverable; and *c*.

That, in consequence of the great rapidity of the process, the prognosis in these cases is of the worst.

Why peripheral prolapse should expose an eye to such a danger, it is not easy to understand. Is the proximity of the ciliary body somehow in play? It cannot depend on the tension and dragging of the iris, for in Case 1 this was nil.

Art. XIX.—*On the Efficient Powers of Parturition.*[a] By J. Matthews Duncan, M.D.

There can be no doubt that among the numerous matters at present occupying the attention of obstetricians, none is more important than the subject of this paper. So evident is the truth of this statement that one cannot but wonder why attempts to arrive at the truth have been, so far as we know, delayed till the present day. It is long since excellent researches of an analogous kind in regard to the force of the circulation of the blood, the power of the ventricles of the heart, were published; yet, such researches do not seem naturally so attractive, nor do they give promise of so valuable practical results as those into the power of labour.

It is well known that the first, and I believe the best, results in this inquiry have been obtained by careful deduction from experiments on the tensile strength of the amniotic membrane. The researches referred to were made quite independently, and published soon after one another by Poppel, of Munich, and by Tait and myself conjointly. Studying this subject, I thought of some other modes of reaching conclusions, such as by observations on the caput succedaneum. Means might be taken to find the force required to raise this swelling in different degrees of thickness. Such an investigation would, no doubt, lead to similar valuable results; but the plan has never been employed. Again, observations might be made to ascertain the force required to rupture the fourchette or the perineum, and thus a fact might be got which would be of service in this inquiry. It is well known to accoucheurs that these parts sometimes offer a successful resistance to all the powers of labour. This resistance, if its force be ascertained, is of course a measure of the power employed; at least it

[a] Communicated to the Royal Society of Edinburgh, 6th February, 1871.

would afford a valuable result as to the higher limit of the power. Like statements might be made regarding the laceration of the margin of the cervix uteri as a test of the power exerted at the completion of the first stage of labour. Many methods were available, but none were, till very recently, worked out.

It is probable that many intelligent and thoughtful accoucheurs had some rough ideas as to the amount of power exerted in parturition. They could not fail, in attending on ordinary labours, to observe the amount of exertion by hand and arm required to keep back the head when too rapidly advancing over a delicate perineum. This power is, under certain conditions, a measure of the force of the labour; but I am not aware that any one has hitherto made the simple dynamometrical experiments proper to decide the amount of force so exerted by the accoucheur.

The problem may be more exactly stated as follows:—If in an unobstructed and powerful labour, the accoucheur, by the directly opposing pressure of his hand on the fœtal head, arrests its progress for one or several pains, he has in the pressure by his hand a force which, added to the small amount required to effect parturition, exceeds all the combined powers of labour in this case. He may then estimate, by dynamometrical experiment, what was the force he used or what force he is capable of applying in the way in which he actually applied it to arrest the progress of labour. This experiment may be varied in different ways, and I shall mention one. Let us suppose a case of rigid vulva, the perineal resistance being overcome, and the head retroceding during the interval between powerful bearing down pains. Now, it is well known that in such a case a little manual pressure from above may be enough to push the head down again upon the perineum or to resist its retrocession, or that the first and painless part of the next pain will make the head that has retroceded again bulge out the perineum before it is forced by the powerful acme of the contractions against the resisting vulva. If, then, the practitioner opposes the advance of the head, even so far as to bulge out the perineum, he must have a nearly exact measure of the force which the labour could bring to bear against the vulvar obstacle. The conditions given for this experiment are introduced in order to secure that friction may be reduced to a trivial amount.

In such experiments or practice, what force does the accoucheur exert? I have a hand well accustomed to such work, and I find, by actual trial with an accurate dynamometer, 50 lbs. to be about

the highest power I can use, situated as I am at the bedside in attendance on a case. I have ample reason, then, in such experience, to believe that very few of the most powerful labours exert a force of 50 lbs.; that an ordinary strong labour is easily arrested by a smaller force than 50 lbs.; that the great majority of labours are accomplished by repeated impulses, whose highest power probably never exceeds 25 lbs. I may add that, in the great mass of short forceps deliveries, the force required from the accoucheur, even when he delivers the head unaided by the natural efforts, seldom reaches 50 lbs. These statements are, to a great extent, either arbitrary or dependent on my skill as an observer; yet I feel very confident of their accuracy.

Again, the intelligent practitioner who has observed a case of difficult labour, finished either by the long forceps or by podalic extraction, could not but form some rough idea of the force he used and compare it with the force which the labour exerted in its nugatory struggles. The force which the accoucheur thus exerted would not be certainly the equivalent of what the labour must have put forth in order to produce a spontaneous termination. It would, no doubt, in most cases surpass the force which the mother must have exerted to produce the spontaneous birth. But it would be, nevertheless, a valuable measurement indicating a force which in such a case the labour failed to produce. Joulin and I have made dynamometrical experiments to make use of such measurements in estimating the highest power of labour.

Another method of advancing our knowledge of this subject has been followed by the Rev. Professor Haughton. This gentleman does not, as his predecessors, examine the effects produced by the powers of labour, and thus get results having a very distinct positive value. He follows a plan which may be justifiable, yet which is difficult and dangerous. He takes an almost opposite method to that used by me. He measures the bulk and the extent of the involuntary and voluntary muscles employed in the function, and from these data he arrives at conclusions which he, in one particular, corroborates by a simple experiment. The results arrived at are statements of the powers of the parts, which are true if his methods are true. Even if his methods are correct, the results are not actual values but possible values or statements of what may be, not of what has been.

These results are very different from those of Poppel, Tait, and myself, and it is one of the objects of this paper to inquire into

their value. In doing this I shall not discuss the method, but merely examine the results, by the aid of any obstetrical light which I can throw upon them.

Before proceeding to this inquiry it is to be remarked that Haughton arrives by his method at new results which the methods of previous observers did not afford the means of reaching. There are, as is universally known, two great forces employed in labour—the uterine contractions and the involuntary and voluntary bearing down. The former of these forces is peculiar to the parturient female; the latter, as Haughton truly observes, is not peculiar to parturition, but is "available to expel feces, urine, or a fœtus." Haughton's plan is, to examine the uterus, measure it, and through this arrive at a conclusion as to its power; then, to examine the muscles which co-operate to produce bearing down, measure them, and through this arrive at a conclusion as to their power. The addition of the two results will, of course, give the power of labour. As I have already said, this is a dangerous and difficult plan to follow, and this is because there is room for error at every step.

The conclusions which Poppel and Tait and myself enunciated regarding the power of natural parturition stand on a completely different, and it appears to me, far more secure, footing. There can, indeed, be scarcely any important objection raised regarding them. The strength of the fœtal membranes is ascertained by experiment. Certain facts are well known regarding the rupture of the membranes generally, and regarding their rupture in the labours in which the membranes experimented on were produced. These two sets of data, when put together, lead, by a process of reasoning which it would be tedious here to recapitulate, to conclusions regarding the lower limit of the power of natural labour, and regarding the power of labour generally, which cannot, so far as I can see, be cavilled at. It is evident that this method tests only the whole or the combined powers of labour. It can afford no hint as to the comparative value of the two forces which combine to produce the power which is to be measured.

Of the results given in Professor Haughton's paper, those which appear to me to be both new and important, are three. I shall first state them, and then proceed to their examination one by one.

I.—The first conclusion is, that "the uterine muscles are capable of rupturing the membranes in every case, and possess, in general, nearly three times the amount of force requisite for this

purpose." . . "It would be a waste of power (adds Haughton) to endow the uterus with more force than I have shown it to possess, for it is not necessary that the uterus should complete the second stage of labour, as the abdominal muscles are available for this purpose; so that by using them, and not giving the uterus more force than is absolutely necessary for the first stage of labour, an admirable economy of muscular power is effected." . . "The extreme force of uterine contraction produces a pressure of 3·402 lbs. per square inch, which is equivalent to a pressure of 54·106 lbs. acting upon a circle of $4\frac{1}{2}$ inches in diameter, which is assumed as the average area of the pelvic canal."

II.—The second of Professor Haughton's new and important conclusions is that the action of the voluntary abdominal muscles "constitutes the chief part of the force employed in difficult labours." . . "The amount of available additional force given out by the abdominal muscles admits of calculation, and will be found much greater than the force produced by the involuntary contractions of the womb itself."

III.—The third conclusion is, that, "on an emergency, somewhat more than a quarter of a ton pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner." . . "Adding together the combined forces of the voluntary and involuntary muscles we find—

"Involuntary muscles, - - -	54·10 lbs.
"Voluntary muscles, - - -	523·65 lbs.
"Total,	577·75 lbs. av."

I.—The first of Professor Haughton's conclusions on which I comment is to this effect, that the unaided uterine muscle can exert in labour a force of 54 lbs.; that this force is employed in dilating the cervix and rupturing the membranes, and that it can or does effect little more.

Now, it appears to me that Haughton limits far too much the use of the power of the uterus. I have no doubt that the uterine efforts not only dilate the cervix and rupture the membranes in most cases, but also do in most cases perform the chief part of the work required to bring forth the child. Although I do not coincide with Haughton in his reflections on the economy of muscular power, I shall not discuss the point therein raised. Yet I cannot avoid saying that, in the present instance, his own statements invalidate his reflections, for he asserts that the uterine

muscle has three times the amount of muscular power required to do the work demanded of it. In endowing the uterus with this great power, Haughton, in my opinion, furnishes conclusive evidence against his own view as to the use of the contractions of the uterus. For I am sure that the great mass of births, even in difficult labours, excluding only the most difficult, is effected and completed by a force less than what Haughton ascribes to the uterine muscle alone, unaided by the voluntary muscles. I am satisfied that the whole combined powers of labour seldom reach above 50 lbs., while Haughton gives the uterus alone a power of 54 lbs.

I do not say that Haughton is wrong in supposing that the uterus can exert a force of 54 lbs. On the contrary, I have no reason to doubt it. But I am sure that, while easy labours require for their whole work a force scarcely exceeding the weight of the child, only a very few difficult labours require for their whole work a force exceeding 50 lbs.

Every accoucheur knows to some degree of exactness the force which is required to restrain the forward movement of the child when there is no special resistance to its advance. This power I have measured approximately by dynamometrical experiments, and I find it to be at the most 50 lbs.—a power less than what is ascribed by Haughton to the unaided uterus. In other words, the uterus and voluntary muscles combined, stimulated to violent effort by insuperable temporary resistance, exert a force greater than is required to complete the labour; yet this force is generally much less than 50 lbs., and possibly never exceeds it.

It is well known to accoucheurs that the great resistance to the progress of the child in the second stage of labour is what is called in obstetrics the perineum. The power of this part I do not know, and guessing is a bad proceeding in a scientific paper. Yet I may venture to say that no perineum would long resist a force of 50 lbs., repeatedly applied—a force less than Haughton ascribes to the uterine muscle.

II.—Haughton's second conclusion is that the chief force in parturition is furnished by the voluntary muscles. The available power of these is (he says) 523 lbs., while that of the uterus is 54. The whole amount of expulsive force of the voluntary muscles is, he says, not usually employed to assist the uterus in completing the second stage of labour; but this does not contradict the conclusion we have ascribed to him. The conclusion is, indeed, for Professor

Haughton, inevitable; for every accoucheur knows that the bearing down efforts, whatever be their actual measured power, are very strong—perhaps as strong as possible, quite frequently in ordinary labours. Besides, Haughton himself expounds his meaning in the following words:—"It is plainly necessary that the first stage in the expulsion of the fœtus should not be entrusted to a voluntary muscle, and hence an involuntary muscle is gradually provided, which takes the initiative and commences the process of parturition, the completion of which is then accomplished by the aid of voluntary muscles, to the employment of which, at this stage, no moral objection can be raised. It is also necessary (if the Contriver be allwise, or if the principle of least action in nature be true) that the involuntary muscle, so produced, should not possess more or less force than is requisite for its purpose. The uterine muscle does not grow, to meet a growing resistance (as happens frequently in other cases), and its precise degree of strength cannot be produced by a tentative process; for in healthy gestation the uterine muscle never tries its force against the membranes it is called upon to rupture until the actual period of parturition has arrived."

The view propounded in these words has great authority on its side, besides that of the quoted writer, for the point therein raised as to the relative powers and uses of the uterine and auxiliary forces of parturition is one that has been much discussed and for a long time. The great Haller, indeed, held opinions which are in accordance with Haughton's view. This renowned physiologist discarded the opinion common in his day, and now almost universally entertained, that the uterus is the main source of the power exerted in every stage of parturition.

Haughton gives no reason for discrediting the general opinion of obstetricians, relying apparently on his conclusions alone regarding the comparative power of the two forces—that of the uterine muscle and that of the assistant voluntary muscles. No doubt, he makes some observations, intended to be corroborative, as to the economy of force and other so-called laws of nature; but such reflections cannot be regarded otherwise than as premature by those who, like myself, do not adopt this writer's hypotheses, upon whose verity their justice depends.

In the course of his concise view of this question in his work on Physiology, Haller twice takes care to express his doubts as to the truth of his own opinions, and he ends by appealing to anatomists

for light upon the subject. This appeal is, at least, ingenuous, for his argument against the ordinary opinion rests greatly upon the uterine fibres, their direction and the direction of the force evolved by them; and as Haller's notions on this anatomical point were very imperfect, and his mechanical ideas equally so, we need attach no weight to this part of his argument. Besides this, however, he has really nothing deserving the name of good evidence on his side. He thinks the effects produced by the expulsive pains greater than the power of the uterus, but this is evidently mere begging the question. So also is his dependence for aid in his argument on a picture of the great struggles of the voluntary muscles.

Authors generally do, as I have said, entertain an opinion opposed to that of Haller and Haughton. They are too numerous to name, and none of them merits special mention; for, so far as I know, no one has distinguished himself by the novelty or elaborateness of his arguments in support of the ordinary view that the uterus is the chief agent in the whole process of parturition, and that the voluntary muscles, whether stimulated by volition or by reflex excitement, are in a secondary position, aiding the uterus indeed, but not supplying the chief force. There is no positive value in an argument of appeal to authority, yet it is evident that the amount of authority against him made Haller hesitate to enunciate his own views; and when we consider the number, the intelligence, and the acute attention of the obstetricians who form a majority, scarcely differing from the whole body, in favour of one view, we cannot but be weightily impressed in its favour.

I must point out that some of the arguments made by obstetric authors to do regular service in defence of their view are very weak or quite vain, and I may cite examples. Cases of parturition, completed when the uterus is prolapsed and is said to derive no assistance from bearing down efforts, are cited. But such cases prove almost nothing, even supposing they are correctly described, for there is in such cases absence of the ordinary difficulties of labour which consist in the propulsion of the child through the pelvis. Cases of expulsion of the child after death of the mother are quoted. But, so far as I have perused them, they are given with a deficiency of circumstantial data such as to invalidate them altogether. Indeed it is, in some of them, not even shown that the uterus acted at all; while in all there is the assumption that the difficulty of birth after death is as great as before it. The like

objections may be made to examples of labour in asphyxia, narcotism, and syncope. It has been also asserted that narcotism by chloroform affords evidence that the uterus is the chief agent in parturition. But I must assert the incorrectness of this argument; and I cannot understand why Haughton should call attention to the influence of this agent, for any argument from it is valid, so far as it goes, only against his views. I have, in a large experience, never seen chloroform inhalation destroy the action of the voluntary muscles. I believe it generally weakens their action; and it is well known that, at the worst, it only weakens the powers of labour. It is not known whether it weakens the uterine action or the action of the voluntary muscles in the greater degree. If it does, as is alleged, when given profusely destroy the action of the voluntary muscles, it certainly seldom completely arrests the progress of labour. Lastly, cases of labour in paraplegic women are cited in favour of the ordinary opinion. But I fear they do not even appear to favour it; and with a view to the present question, they cannot be held as settling anything, seeing we do not know what influence paraplegia may exert on the uterus itself. Besides, the cases are insufficient in every way.

The arguments on which I place chief reliance are the following:—

1. The great power of the uterus felt by the hand of the accoucheur, as in the operation of turning, long after the rupture of the membranes.

2. The great and sufficient power of the uterus, observed in cases where the action of the voluntary muscles is weak or restrained.

3. The regulating influence of purely uterine pains on the progress of the second stage of labour.

4. The supremely important demand for, and presence of, powerful uterine action after the expulsion of the child.

5. The arrest of the progress of labour by inertia of the uterus. The argument from this appears to me unanswerable, for the condition often occurs when there is certainly only the slightest possible resistance to the progress of the child—when the mother evidently desires the completion of labour, and bears down violently with this object in view.

6. In cases of uterine inertia, such as are above described, the practitioner may find, by pulling with the forceps from below, or by pushing with the hands from above, in the absence of all parturient

effort, whether of the uterus or of the voluntary muscles, that a very small force, say not exceeding the weight of the child, is sufficient to finish a labour upon whose progress violent bearing down efforts have had no effect.

7. The circumstance that, were the voluntary muscles the chief agents, expulsion of the child would be in great part a voluntary act, which it certainly is not.

8. The asserted completeness of the function of parturition in animals in which the assistant bearing down efforts are annihilated by opening the abdomen; the process being carried on by their uterine and vaginal muscles, which are weak when compared with those of women.

Baudelocque and Velpeau[a] relate cases which appear to show that woman has very rarely voluntary power over the progress of parturition, for a time. Such cases offer no difficulty when regarded with a view to the present question. They are explicable in more ways than one; and an illustrative statement is, for my present purpose, quite sufficient. Every experienced accoucheur has seen cases where voluntary increase of bearing down has sufficed to expedite labours which, if the women had been left in a sleepy, lethargic condition, might have been protracted for an indefinite length of time.

There can be no doubt that the uterus is a very powerful agent in expelling the fœtus from its cavity into the world; that it is not the sole agent, and that it is assisted by the action of the voluntary muscles. Though I have not proved absolutely that the uterus is the chief agent in the performance of this function, yet I have no doubt that it is so; and I think that the arguments which I have adduced give this belief of the profession the highest degree of probability. The belief does not imply that the aid afforded by the voluntary muscles is inconsiderable or unimportant. It only renders it quite incredible, that while the power of the uterus is 54 lbs., that of the voluntary muscles can be 523 lbs.

III.—Haughton's conclusion, on which I wish, last of all, to comment is, "that, on an emergency, somewhat more than a quarter of a ton pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner."

In my work entitled *Researches in Obstetrics*, to which Professor Haughton refers, I have discussed carefully but briefly this point,

[a] Traité complet de l'art des accouchements. Ed. Bruxelles, p. 227.

and announce the conclusion that the comparatively small figure of 80 lbs. gives the highest power of labour; and I quote Joulin, who estimates it as somewhat above 100 lbs. I do not deny that in exceptional circumstances a few lbs. above 80 may be reached, but I feel pretty sure that seldom, in the history of woman, has the figure 80 been attained, whether on an emergency or not. This conclusion is arrived at by experiment and observation—experiments on the force required to pull a child through a contracted brim of pelvis; observations of the force used to complete a difficult labour which nature, in its most violent throes, has failed to accomplish.

Every accoucheur will, I suppose, readily admit that in a case of delivery by podalic extraction the surgeon can exert a great deal more force to bring the child into the world than the most energetic labour can. Now, in these circumstances the surgeon can use no force nearly reaching a quarter of a ton. A very much smaller power would rend the luckless body of the child in pieces.

Such a power as a quarter of a ton does, in my opinion, represent a strain to which the maternal machinery could not be subjected without instantaneous and utter destruction. To speak of a rigid perineum resisting such a power, or the fourth part of it, would be ridiculous. The possession and use of a considerable portion of such a power would render the forceps and cephalotribe weak and useless instruments; the mother could bray the child as in a mortar and squeeze it through a pelvis which would, under other circumstances, necessitate Cæsarean section. Such a power would, if appropriately applied, not only expel the child, but also lift up the mother, the accoucheur, and the monthly nurse—all at once. It would be dangerous not only to the mother and the child; it would imperil also the accoucheur. It has been calculated for me that if this force were applied just as the chief resistance to delivery was overcome, the child would be shot out of the vagina at the rate of thirty-six feet per second![a] The blow it could thus inflict would be equal to the shock produced by the fall of the child from a height of twenty-one feet.

In an early part of this paper I have said that the method of inquiring into the subject which Haughton adopts is both difficult

[a] In making this calculation the child is taken as 7 lbs., the pressure as 580 lbs.; and it is supposed to be exerted through a space of three inches—measurements which are fair statements of the case. It is further supposed that the friction is negligible when compared with the forward pressure. This is certainly the case if the forward pressure is nearly as much as is stated by Professor Haughton as possible.

and dangerous; and I think I have said enough to show that danger has not been avoided. There must be error in Professor Haughton's calculation of the power produced by the action of the voluntary muscles, or there must be error in judging of the application of this power to the accomplishment of the function; or there must be error in both. I shall not attempt to show where the error lies, but its occurrence does not astonish me; for any one who has studied the difficult subject of the retentive power of the abdomen will recognize the difficulty of reaching conclusions as to the power of labour by Haughton's method. It is highly probable that the power of the voluntary muscles is dissipated—perhaps in compressing intestinal gases, perhaps in consequence of being misdirected.

Whatever may be the real source of error it is highly desirable to find it out, in order that we may, by more accurate proceedings, arrive at the true results which Haughton hoped to reach.

ART. XX.—*Personal Experience of Lithotomy in India.* By WM. CURRAN, L.R.C.P., Edinburgh; M.R.C.S., England, &c.; Assistant Surgeon, Army Staff.

THE subject of calculous disorder in India is a comprehensive one from whatever point we may regard it, but I have neither the time nor the materials that would enable me to deal with it in its entirety, and it is one which will easily admit of delay. Numerous ably written papers on the subject are to be found in the medical journals published in that country, but these are not now accessible to me, and even if they were, they could scarcely be turned to account in this place. I prefer discussing the question on its own merits, and from my personal point of view; and though it may, when thus divested of its native surroundings, appear somewhat stilted and individualized, it will gain in interest what it loses in importance, and have the further advantage of being supported by facts which rely on other than personal evidence for their guarantee. I will, for these reasons, confine myself to describing here, as concisely as I can, what I saw or did myself in the matter, while in India; but to enable me to do so with more effect, I will not hesitate to avail myself of the experience and observation of those gentlemen from whom I received cases, and to whose courtesy and kindness I owe it, that I am in a position to speak on the point at all.

Viewed in this light, the rehearsal at such a distance of time and place, may appear to lack originality, and some even may regard it as open to the imputation of being biassed by prejudice, or exaggerated by favour. It may be so, though I am not conscious of being influenced by either, and no fear of consequences will ever prevent me from doing justice to the merits of a friend. I do not pretend to or aim at being original. I merely undertake to state here, in plain unvarnished English, the results of my own experience and observation, that I purpose doing to the best of my ability, and I am content to leave the rest to the indulgent criticism of my readers.

When I first went to India, medical officers of the Queen's as distinguished from those of the native army or local establishments, were eligible for appointments under the Indian Government, which frequently placed them in some of the smaller outlying stations, in charge of dispensaries, gaols, and other civil duties, but these openings exist no longer, as the line of demarcation between the two departments has been drawn hard and fast, and, limited as my experience is, it is not likely to be equalled by any of my successors. The statistics I am permitted to produce, will show that the success claimed for or by Indian surgeons is not exaggerated, and I am in a position to give some account of an operation which must be new to many of my readers, and which is only known by name to curious searchers of surgical bibliography. For these reasons, if for no other, the subject will justify some discussion, and its interest and importance will scarcely be questioned. I will only add, that, as I had no separate charge of my own that would enable me to secure a regular flow of cases, and as I was indebted entirely to the kindness of others for those I have received, I kept no very minute or searching record of my proceedings. My custom was to write a short account of the case and its surroundings on a scrap of paper as soon as it was completed, and to enclose the stone in that scrap. The white ants have eaten many of these, time and the vicissitudes to which I am subject through my office, have destroyed others, and I am now dependent for information on a few scattered notes of uncertain date and questionable authenticity which have survived the effects of change, and are only decipherable with difficulty by myself. My memory is, however, a good one. I have done my best to exercise it by frequent walks in the wholesome atmosphere of facts, and if I cannot promise entire accuracy in my statements, I can at least say that I have endeavoured to escape error and avoid miscalculation.

That stone in the bladder prevails to a larger extent in the peninsula of Hindoostan, and especially so throughout its central and upper provinces, than perhaps anywhere else of equal extent in the world, is a fact which admits of little doubt. I have been or served in most of the principal stations between Calcutta and Peshawur, including the Saugor district and Bundlecund, and have found the same diathesis, and heard the same story told in all. I believe, however, that the prevalence increases as we approach the hills, but why this should be so, if it is so at all, is more than I can undertake to say, and, indeed, the causation and etiology of calculous disorder in India has never, to my knowledge, been satisfactorily accounted for or explained. Some maintain that its great prevalence is owing to the quantity of lime salts that obtains in the drinking water, and stone is particularly common in the hilly districts near Abbottabad, Almorah, and other parts of the Himalayas, in which limestone enters largely into the geological formation. But this material does not operate as a cause of calculus elsewhere, and stone prevails in equal abundance in parts of the plains, in which lime enters but feebly into the composition of the water. Again, the diathesis is almost, if not altogether unknown in other parts of the world in which lime forms a principal ingredient in the water, as would appear from the writings of Dr. Livingstone, who, after alluding to the rarity, or rather almost entire immunity of the Bakwains and other tribes of Central Africa from syphilitic disease, says, at page 128 of his very interesting "Missionary Travels and Researches":—"Equally unknown is the stone in the bladder and gravel. I never met with a case, though the waters are often so strongly impregnated with the sulphate of lime that kettles quickly become encrusted with the salt, and some of my patients who were troubled with indigestion, believed that their stomachs had got into the same condition." He adds, "this freedom from calculus would appear to be remarkable in the Negro race, even in the United States, for seldom, indeed, have the most favoured lithotomists there ever operated on a Negro." This is another puzzling point of pathology which calls for elucidation, and it does seem strange that the dark man of Africa and America should be almost entirely free from a disease to which his darker complexioned Aryan brother of Asia is so liable. A mere difference of race can scarcely be said to account for the fact, for that fails to secure any such or similar exemption in other instances, and whatever effect lime may have in

producing or aggravating the tendency to stone that exists in India, it is clearly inoperative in that direction elsewhere.[a]

Others hold that it depends in great measure, at least, on the quality of the food used by the natives, and the immunity in this respect enjoyed by flesh-eating Eurasians and Europeans would seem to lend some support to this hypothesis. But stone is very prevalent in certain parts of Persia, and also, but to a lesser extent, in the interior of Cabul and along the banks of the Oxus—as I learned from traders—where the inhabitants are all Mahomedans, who eat flesh and live in some other respects like Europeans. I have heard it more than once ascribed in conversation with friends in India, to the predominance of the vegetable or coarsely powdered materials of their food, and to the absence of that regulated admixture and combination of those nitrogenous elements that are considered essential to health, and which are so little cultivated by the poorer class of natives. This view has the support of analogy in its favour, inasmuch as a similar prevalence obtains in parts of the Highlands of Scotland, in Norfolk in England, and near Malaga in Spain, in the first and last of which places, the poor, as is well known, live largely on barley meal and the products of the vine, while the want of apples, pears, cider, and other articles of that description has been said to conduce towards its causation and development in the latter. Certain it is that the miserable mills used by the poorer class of natives are but ill adapted for grinding, and the kind of corn or rather pulse they use has a hard, horny, coriaceous rind. But it is not easy to see how particles of this could enter the bladder, and yet an impression to that effect prevails largely elsewhere. When on a visit to Malaga, in June, '58, I called on the late Dr. Sutliffe, who then practised there, and who possessed a considerable local experience and repute. He showed me several specimens of calculi he had extracted on the spot, and he assured me that the disease was comparatively common in the neighbourhood. He unhesitatingly ascribed its prevalence to the wine which is manufactured in the vicinity, and which the peasantry are so fond of, and to the use of a species of bread which is made of maize and the husks of the vine, and which is of a coarse, indigestible and gritty nature. Whether any similar effect can be ascribed to the barley meal of the Highlanders, I cannot say, but stone

[a] Dr. Cameron informs me that lime is found in considerable quantity in the drinking water in this country, and he added that he believed stone is less frequent in Dublin than it used to be before the introduction of the soft Vartry water.

prevails largely, I am told, in the mountains near Aberdeen, and a friend informs me while writing this, that he heard the use of treacle and dumpling by the inhabitants assigned as a cause for its frequency near Norwich. How far such a diet is calculated to affect digestion or perpetuate the tendency towards calculous formation that clearly exists in the above-named localities, I cannot undertake to say, but the facts admit of no question, and I am not aware of any better explanation of them.

Another hypothesis exists which deserves a passing mention, and as the custom on which it is based prevails universally throughout

[a] Since writing the above I have come across a communication by Dr. Crisp, on "Urinary Calculi in the Lower Animals," which appeared in the "Transactions of the Pathological Society of London," Vol. xxi., p. 427-8, and which contains matter so pertinent to the question under review, and so generally confirmatory of my own impressions, as to justify me in reproducing it here. After stating that "the constant use of Norfolk and Suffolk dumplings has been assigned as one cause of the greater prevalence of this affection," and saying that he "thinks it is not an unlikely one," he adds, "Dr. Greenhow (Annals, 1866) wrote to forty different surgeons in the North-West provinces of India, for the purpose of ascertaining 'the nature of the operation, amount of division of the prostate, use of tubes, the comparative success of lithotomy and lithotrity, and the effect of chloroform?' From twenty surgeons he received answers, and these gentlemen, most of them attached to dispensaries, had operated on 1,851 patients, including 91 females. Dr. Courtney had 201 cases in twelve years, Dr. Keernander 28 cases in one year, Mr. Gorgaon 143 cases in six years, Mr. Newton 48 cases in four years, and the cases of the remainder occurred within a short period. Of these 1,851 examples, 1,160 occurred among Mussulmans, and 551 were Hindoos. Taking the population into account the numbers are about equal. The youngest patient was one and a quarter years. The largest stone weighed 11oz., the smallest 3 grains, and the largest number of stones was 12. The mortality after operation, 1—6·93. Scurvy prevailed to a great extent both among Hindoos and Mussulmans. An interesting statement is made respecting the analysis of a collection of calculi from this district of India. Only two kinds were found, bone calculus, and earthy phosphates and ammonia (p. 14). *No lithic acid nor triple phosphate calculi were found.*"

In another communication, in 1868, by Dr. Garden, an analysis is given of 831 cases of urinary calculus—including females—which occurred at the Saharnupose Dispensary (Calcutta Presidency) during a period of 18 years. The mortality after operation was 1—7·63—two or three per cent. less than the mortality in the United Kingdom. Of these 577 were Hindoos and 254 Mussulmans, forming, as in the example already quoted, but a slight difference when the number of the two castes is taken into account. The chief causes assigned by Dr. Garden are exposure, bad grain, bad digestion, and rheumatic complaints. An analysis of 260 calculi examined forms a remarkable contrast with that already quoted:—"15 were fusible, 1 triple phosphate, 3 phosphate of lime, 81 uric acid, 55 urate of lime, 23 urate of ammonia, 72 oxalate of lime. Of these 58 were pure calculus, uric acid 26, fusible 10, oxalate of lime 10, urate of lime 5, urate of ammonia 1, triple phosphate 1." I will only say, with reference to the above, that it appears to me the relative position of the Mussulmans and Hindoos ought to be changed in the first part of this quotation, and I can scarcely help thinking it stood differently in the original.

the East, it is not so flimsy or improbable as it may at first sight appear. The natives of India, as every one who has visited that country must know, squat down on their haunches[a] during the act of micturition instead of standing up as we do in Europe, and there can be no doubt that such an attitude is not as favourable to the free and entire evacuation of the contents of the bladder as the one we assume is. On the contrary, it seems to favour the retention of urinary debris and salts in that viscus, and when we consider that "urinary concretions may be formed either in the kidney or in the bladder, and are almost always the result of the deposition and retention of a urinary sediment in some portion of the uropoietic tract,"[b] we will be able to appreciate its bearing on the question, and influence in propagating the disease. It may also, perhaps, serve to explain the great exemption from this complication which women everywhere enjoy. Dr. Day adds, "around the nucleus thus formed, additional matter is gradually deposited, till at length the concretion may attain an enormous size." Just so, and if to the action of this custom we add the agency of the other influences mentioned above, we will be able to suggest an explanation, or, at least, lessen the difficulty that now exists regarding the wide-spread

[a] Those who have read the interesting "Journey to Mecca and Medina," written by Captain Burton of the Indian army, and now English Consul in the Brazils, will remember an incident in connexion with this attitude, which is as amusing as it was nearly being tragical. It is wonderful in how many small details of habit a difference is to be observed between the European and the Asiatic. An observation of these has led me to heartily agree with Mr. Palgrave when he says that the attempts made by European travellers to personate or pass for natives by assuming the garb of Dervishes, Fakeers, Merhauts, &c., are miserable failures. Mr. Kaye—than whom there can be no better authority on such a point—says substantially the same thing in his life of Eldred Pottinger, and any one familiar with the East, and who will take the trouble of comparing Sir Alexander Burnes and Mr. Vambery in their costumes, with natives similarly dressed, will see at once the difference. I can say for myself that I never saw a European who could talk and act in all respects like a native, but the fear of consequences, the innate courtesy of the Oriental, his fatalism or *sang froid*, and perhaps also his doubt as to the sanity of those members of our race who undertake journies and embark in enterprises for which he can find no better explanation, constitute their claims to protection, and some are, I believe, sped on their way through feelings of charity, pity, or contempt.

[b] Day's Physiological Chemistry, page 377, Mr. Erichsen speaks to the same effect in his Clinical Lecture on Lithotomy in Recurrent and Multiple Calculus, in the *Lancet*, for March 18th, 1871. He says, "just as the water of a petrifying well will give rise to calcareous deposit on a twig left in it, so will the urine under certain conditions, local or constitutional, give rise to a phosphatic deposit round any body on which crystals can form, and for this purpose a clot of blood or a shred of cellular tissue will suffice."—See also Neubauer and Vogel's Guide, Sydenham Society, p. 431.

prevalence of the condition here contemplated. Neither cause will suffice of itself to produce the disease, but assuming the existence of a diathesis, one can easily see how the three may concur and conduce to the same result, and at any rate the coast is clearer in regard of a remedy. But whatever the cause, there is no doubt as to the fact, and the following table, which is taken from a statement that was prepared for circulation among the officers of his circle by the late Deputy Inspector General of Hospitals, John Wilkie, M.D.,[a] and which was kindly placed at my command for the purpose of this inquiry by a friend, will show this and show also the advantages of India as a field for the cultivation and practice of the higher branches of surgery. It will further, I think, tend to show that the prevalence increases as one approaches the hills; but be this as it may, the document is a highly suggestive one, and though shorn of much of its interest by the absence of details, its perusal is calculated to enhance the repute of our Indian brethren, and confirm the impression that prevails respecting the vital capacity, endurance, and great powers of recovery possessed by the natives. As such it is subjoined here, and I will throw in under the head of remarks such brief, explanatory notes as space may allow, or as I may find absolutely necessary for its elucidation:—

[a] Dr. Wilkie sums up the above as follows in a letter to a friend which was kindly placed at my command, and which is dated Meerut, 26th July, 1863:—"From the half-yearly returns of dispensaries it appears that at Delhi there have been 91 capital and important operations, of which 22 were for lithotomy. At Shahjehanpore, 48 with 12 lithotomy. At Budaon, 78 with 39 lithotomy. At Bareilly, 89 with 35 lithotomy. Bijnore, 25 with 6 lithotomy. There have been 180 cases of lithotomy during the past six months." In other words, if we assume that the population of the Meerut Circle equals that of Scotland, and put down both roughly at three millions, we will be able to form a pretty good estimate as to the percentage and prevalence of stone in the former. I doubt, however, if Scotland could produce half the number in double the time. Another friend discussing the subject with me said:—"In a population of, say, a million, forty or fifty cases of stone may occur in a twelvemonth, and one of these might, *perhaps*, be a female." It will be seen from what is said elsewhere that the import of the italicised word *perhaps* would be more clearly defined by a note of interrogation.

A Comparative Statement of Operations performed in the MEERUT CIRCLE, *and prepared from the Half-yearly Returns of Dispensaries in that District, for the Six Months ending 30th June*, 1863.

Districts	Stations	Operations				Remarks
		Lithotomy	Deaths	Capital & Important	Minor	
Delhi,	Delhi,	22	2	69	320	The district contains, according to Thornton, an area of 789 square miles, and its population amounted in 1853 to 435,744. Of this number 171,694 are returned as Hindoo agriculturists, 144,371 as Hindoo non-agriculturists, and 18,917 are Mahomedans. Upwards of one-third of the entire population is concentrated in the City of Delhi, and is pretty equally divided into Hindoos and Mahomedans. The soil is sandy and barren, and remarkable for its saline efflorescence, and the wells for the brackishness of their water. A large portion of the supply is obtained from a canal which begins 70 miles away.
	Goorgaon,	—	—	14	67	
Allygurh,	Allygurh,	3	—	3	390	The district embraces an area of 2,149 square miles, and contains a population of 1,134,565. The soil is alluvial, and the principal productions are wheat, barley, millet, and pulse. It is freely irrigated from the Ganges canal, and though there is a prolonged elevation of surface in the centre there are no high hills anywhere.
	Hattrass,	—	—	—	—	
	Khyr,	—	—	—	—	
	Secundra-Rao	—	—	—	—	
Meerut,	Boolunshuhur,	8	—	7	136	The Meerut district is about 57 miles in length from east to west, and 48 in breadth; its area 2,332 square miles; and its population upwards of 1,135,072, the majority of whom are Hindoo in creed and non-agricultural in occupation. The soil is sandy, with a subsoil of kunkar or calcareous conglomerate, and "the vicinity of the mountains, the comparatively high latitude, and considerable elevation, render the district one of the healthiest parts of the plain of India."
	Meerut,	11	—	15	525	
	Haupper,	—	—	—	167	
	Moozuffurnuggur	2	—	16	185	
	Roorkee,	4	—	8	48	
Deyrah	Saharunpore,	16	3	2	273	The Dehra Doon is a fertile valley at the south-western base of the lowest ridge of the Himalayas, the "soil is in general a deep rich mould, though in some places composed of shingle or gravel swept down by the torrents from the mountains," and it produces rice, grain, and maize in large quantities The ridge on which its famous sanitarium Mussorie is situated consists of beds of compact limestone, alternating with others of soft slate, which resembles the mountain limestone of England. In some places trap rock makes its appearance, and the district is everywhere traversed by or irrigated from the Ganges and Jumna.
	Deyrah,	4	—	12	309	
	Khalsee,	—	—	—	—	
	Mussooree,	—	—	8	103	
Shahjehanpore	Shahjehanpore,	12	2	36	338	A large Mahomedan city and district in which, however, the Hindoos predominate. These live for the most part by agriculture, and their children are very subject to stone.
	Goolurrea,	—	—	—	123	
	Kutra,	—	—	—	116	
Budaon,	Budaon,	39	3	39	750	An almost purely agricultural district, in which the rice-eating Hindoos constitute nearly six-sevenths of the population. The prevalence of stone in this and the following places which are near the hills, would seem to lend support to the impression formed above, and which implies that the tendency thereto increases as we approach to the Himalayas.
	Bilsie,	—	—	5	255	
	Bisowlee,	—	—	16	211	
	Datagury,	—	—	12	219	
	Gonour,	—	—	4	207	
	Saheswaw,	—	—	12	207	

Districts	Stations	Operations: Lithotomy	Deaths.	Capital & Important	Minor	Remarks
Bareilly,	Bareilly,	35	4	54	736	An important military station, and the capital of a large agricultural district, which numbers a population of 1,378,268, whose alluvial soil is very fertile, and which is watered by rivers that rise in and bring down large quantities of gravel, sand, and other similar material from the neighbouring hills.
	Amlah,	—	—	—	215	
	Bessulpore,	—	—	—	305	
	Baharee,	—	—	—	295	
	Philibeet,	—	—	—	305	
	Rampore,	—	—	—	60	
Moradabad,	Moradabad,	18	1	19	1,411	A densely populated and very fertile district, in which, though the quality of the water is good, and that fruit and vegetables grow in abundance, yet stone is very prevalent, but I have heard no adequate reason assigned for this.
	Kaseepore,	—	—	—	—	
Bijnore,	Bijnore,	6	2	19	394	Near the hills, and therefore, as I think, the subject and seat of a large predominance of calculous disorder.
	Nugeenah,	—	—	—	49	
	Nujeebabad,	—	—	—	91	
Nynee Tal,	Nynee Tal,	1	1	1	54	—
	Huldwalee,	—	—	—	—	
	Kaladungee,	—	—	—	—	
Almorah,	Almorah,	8	—	15	251	In the hills, and limestone enters largely into the local geological formation.
	Petoragurh,	—	—	—	—	
Gurwhal,	Bhe Kya,	—	—	—	—	Do., but there is no European officer at either of these stations, and the returns are consequently worthless.
	Chamolie,	—	—	—	—	
	Josee Malh	—	—	—	—	
	Kurumparag,	—	—	—	—	
	Melchourie,	—	—	—	—	
	O'Keemath,	—	—	—	—	
	Sreenuggur,	4	—	5	49	
	Grand Total,	193	18	391	9,225	

This shows a percentage of 9·3 of deaths to recoveries, or about one death to every eleven and a half persons subjected to operation, a result which, though not altogether as favourable as might be expected, is yet considerably in advance of that obtained in England and Europe generally, where, according to the authorities quoted by Erichsen, it varies between one in six and one in eight, the latter being, according to him, the latest and perhaps most favourable average hitherto obtained. And that this result holds good on a larger scale, and when tested, by larger numbers will appear further from the table subjoined which was drawn up by the same gentleman from similar or equally authentic sources, and which extends over a period of time and includes a variety of cases that are amply sufficient for purposes of comparison. They both, however, labour under the grave drawback of being wanting in particulars, and of saying nothing about the age, caste, condition, or sex of the sufferers, but stone is so rare among women in India, that we may safely exclude them from the calculation; and as to caste, that did not appear to me to affect the result, or aggravate a tendency to the disease in any of the cases with which I had to deal. The rich Brahmin who was clothed in purple and fine linen, and who fared

sumptuously every day, on rice, milk, butter, and other equally digestible articles, was as subject, in proportion to the numbers of his highly-favoured order, as the poor pariah sweeper or chumar, who went about in rags that barely sufficed to cover his nakedness, and fed on such offal as his miserable daily pittance could procure, or as the cold and casual charity of his neighbours would allow. The same may be said, with perhaps some modification, of the flesh-eating Mahomedan, whose drink, like that of his Hindoo brother, was the same "pure element" from the brook, and whose religion is equally exacting in respect of bodily purification and cleanliness. The children of both seemed to me to suffer in equal proportion, and I never met a woman of either sect who laboured under the complaint. Not that women are altogether exempt, for such is not the case, but the number of them that suffers bears no proportion to that of the men, and such is the innate bashfulness or mistaken modesty of the Hindoo female, that many, I am sure, would rather suffer in silence than submit to an examination. A friend of mine of many years standing in India, and a very successful lithotomist too, assured me that he had only met two cases in women during his time, and I never knew a surgeon there who had seen more.

On the other hand, very old men are often found affected with stone, which may have caused them no inconvenience till displaced by accident, or otherwise interfered with; and very advanced age is not necessarily a bar to operative interference, provided that the bladder has not lost its contractility or the kidneys been its first seat; and in any case the simpler the expedients and appliances employed the more speedily is the operation performed, and the more satisfactory and enduring are the after-treatment and results. So selfish is the nigger *pur sang*, so impatient of restraint, and so anxious to make the most of his time and opportunities, that he is willing to incur the gravest risks, nay, even to expose his life to the hazard of a throw, rather than wait patiently for improvement, or accept with equanimity an alternative which offers him a better chance of recovery. Hence lithotomy is, and ever must remain, the operation for India; and so reckless are some of the natives of consequences that they do not give even that a fair chance. It is no uncommon thing for patients there to "bolt" before their cure is half complete; and such is the impatience of mothers, or so great their love for their children, that they often try to steal them away as soon as the stone is extracted. Expectant measures will

never find favour with a people who prefer remedies that appeal at once to their senses; and lithotrity, with its more complicated details and numerous sittings, will never commend itself to their untutored understandings, longing for personal freedom, and unreasoning dislike of whatever imposes a restraint or entails an obligation.

The following general statement includes the number of cases and their results, that were subjected to operation in the Meerut circle of Supervision during the years '61-'62, and first half of 1863:—

Period	Lithotomy	Deaths	Capital and Important	Minor	Remarks
1st half of 1861, -	180	19	369	3,842	By capital and important are meant, I presume, amputations, excisions, the ligature of arteries, removal of tumours, &c.; such operations, in a word, as involve danger to life, and call for extra watchfulness and skill on the part of the surgeon, or entail greater expenditure by the medical department on behalf of the public.
2nd half of do., -	118	9	256	4,834	
1st half of 1862, -	160	12	432	5,532	
2nd half of do., -	145	15	284	8,357	
1st half of 1863, -	193	18	391	9,225	
Total, 2½ years,	796	73	1,732	31,790	

The results differ so little in this instance from those given above as to save me the trouble of adding anything by way of explanation; they are, in fact, substantially the same in both cases, and being doubtless based on or derived from similar premises, may be set down as, in round numbers, deaths 9·1 per cent., or about one death to every eleven persons; in other words, nine men died, according to the above-quoted return, out of every hundred who were treated by lithotomy; or, better still, for every sufferer who succumbed to shock, exhaustion, peritonitis, hæmorrhage, or other cause incidental to this operation, among a hundred cases, 90·9 recovered. This is a percentage which, as far as I can determine, has never been exceeded on an equally large scale elsewhere. If we could only know the numbers operated on by

European surgeons, and base our estimate on them alone, I am quite certain the result would appear more favourable. Many, perhaps the greater part, of these were treated by Bengalee sub-assistant surgeons from Calcutta, and some were doubtless left to the native doctor of the North-West. This is an item and an element which should not be lost sight of in the calculation; and even with this, the result is such as European surgery in India need not be ashamed of.[a]

And with regard to this distinction, I have often regretted the absence of reliable statistical information on the subject of lithotomy in India, and have endeavoured from time to time, as opportunity offered, to collect such myself. But it is not easy to procure any, as the disease is, comparatively speaking, so common as to attract but little notice; the genius loci is not favourable to exertion, and familiarity in this, as in other matters, begets contempt. I am, however, satisfied, from the conversation I held with my brethren, that the case stands pretty much as I have stated it; and I have now before me a note from an old friend, dated August 3rd, 1863, in which he says:—"I have operated on about eighty cases of stone since I came here, and of these I have only lost three, and one of these was *in articulo* on admission." He added that the

[a] The following is Sir Henry Thompson's summary of the results obtained by him from an investigation into the causes of death after lithotomy, which is, as far as I can determine, the completest and most comprehensive in our language. I will leave it to speak for itself:—"A table in another part of this work shows that, from the first to the fifth year the deaths are about one in fourteen; they then decrease, so that between six and ten years inclusive they are only one in twenty-three or twenty-four cases. Between eleven and sixteen the mortality gradually rises to one in nine and a-half cases, and from the sixteenth to the twentieth year to one in seven cases."—*Practical Lithotomy and Lithotrity*, pages 93-4. Again, he adds elsewhere:—"As we pass from twelve to sixteen years the death-rate rises; for during the period when puberty is declaring itself, as sexual activity becomes developed, we find the increased susceptibilities thus aroused, raising the mortality to one in nine and a-half, and from thence to the twentieth year, to one in seven cases. It improves but slightly up to the thirtieth year, being until then about one in eight cases. As manhood advances and the strength increases, the death-rate diminishes to one in ten and a-half between the thirtieth and thirty-eighth year. But during the succeeding ten years, organic morbid changes beginning to set in, and the influence of continued depraved habits to tell on the constitution, the rate rises to one in six. These causes become more active, and, at the same time, the powers of life diminish as age progresses from fifty to seventy years, the rate rising to one in four and three-quarters between forty-eight and fifty-eight, and gradually to one in three and three-quarters between fifty-eight and seventy; and finally to one in little more than three between seventy and eighty."—*Ibid.*, pages 142-3.

operation was almost always successful in his hands in children, and I am glad to be able to reproduce here, in a more authentic form, the valuable experience of my friend, Dr. Grant, of Futtehgurh, to whose courtesy I owe the greater part of my own cases, and whose uniform kindness and unswerving friendship I take this opportunity of acknowledging. He has placed the fruits of his labours unconditionally at my command; and I feel that I would be detracting from their value, and, at the same time, doing an injustice to my own sense of his capacity and skill, were I to preface them with a word of justification, or even of praise. They speak loudly enough for themselves; and I am well content they should do so without any aid or introduction from me.

List of Operations for Calculus Vesicæ performed by Dr. G. Grant at the Futtehgurh Branch Dispensary of Futtehgurh, N.W.P., India.

Serial Number	Age of Patients	Caste	Date of Operation	Date of Discharge	Duration in Hospital	Weight of Calculus	Remarks
	Yrs.		1862	1862	Days	oz. dr. gr.	
1	32	Chamar	Mar. 15	April 18	34	– 6 6	Shoemaker
2	12	Kesan	„ 21	„ 30	40	– 4 40	Farmer
3	15	Gararea	April 1	„ 30	29	– 1 40	Shepherd
4	15	Birhamun	„ 15	May 15	32	– 2 26	Priest
5	5	Birhamun	May 2	June 5	34	– 5 20	„
6	36	Mosulman	June 9	July 16	38	– 3 40	Mahomedan
7	4	Kache	„ 28	„ 22	25	– 1 25	Peasant
8	14	Lalee	Sep. 10	Oct. 8	29	– 3 –	„
9	15	Kormee	Oct. 19	Nov. 27	32	1 4 –	„
10	82	Birhamun	„ 19	„ 23	36	– 1 40	Priest
11	4	Gararea	„ 25	„ 20	46	– 1 45	„
12	32	Kesan	Nov. 15	Dec. 27	43	– 1 40	„
13	30	Lohar	„ 20	„ 12	23	– – 30	Blacksmith
14	12	Kesan	„ 21	„ 18	29	– 2 55	„
15	12	Banea	„ 25	„ 27	34	– 2 34	Grain seller
16	32	Dhobee	„ 30	„ 30	30	1 6 30	Washerman
				1863			
17	8	Chamar	Dec. 15	Jan. 4	31	– 3 –	
			1863				
18	2	Chamar	Jan. 2	Feb. 5	35	– 1 –	
19	11	Rahar	„ 16	„ 14	31	– 1 46	Porter
20	20	Chamar	„ 26	„ 28	35	– 7 20	„
21	50	Gararea	„ 28	„ 16	20	– 6 35	„
22	5	Gararea	„ 28	„ 27	30	– 2 40	„
23	22	Banea	Feb. 10	Mar. 13	34	– 7 –	„
24	2	Babna	„ 10	„ 10	30	– 1 48	Unknown to me
25	30	Kesan	„ 11	„ 16	36	– 4 10	„
26	32	Birhamun	Mar. 11	April 10	31	– 4 50	„
27	40	Thakoor	„ 21	„ 18	29	2 2 10	High caste, priestly order

Serial Number	Age of Patients	Caste	Date of Operation	Date of Discharge	Duration in Hospital	Weight of Calculus	Remarks.
28	45	Kurmee	Mar. 29	April 29	31	1 – 20	High caste, priestly order
29	2	Kurmee	May 5	June 8	35	– 1 36	„
30	7	Ahier	„ 7	„ 1	25	– 1 15	Cow herd
31	12	Gararea	„ 23	„ 28	37	– 5 40	„
32	5	Ahier	June 3	„ 30	27	– – 45	„
33	41	Ahier	„ 22	July 30	38	– 4 30	For the rest, the sub-
34	50	Ahier	July 20	Aug. 30	41	– 4 4	divisions of caste are
35	13	Chamar	Aug. 1	„ 30	28	– 3 30	so numerous and un-
36	12	Chamar	„ 10	Sept. 2	23	– 1 –	important, and withal
37	6	Kache	„ 29	„ 20	23	– 1 –	are so entirely based
38	50	Ahier	Sept. 7	Oct. 1	25	– 7 6	on local usage or mere
39	17	Mosulman	Oct. 12	Nov. 8	28	– 2 6	ceremonial observan-
40	8	Kahar	„ 20	„ 18	30	– 2 20	ces, which have no
41	6	Birhamun	„ 29	„ 25	28	– 1 10	counterpart in Europe
42	7	Lohar	Nov. 8	Dec. 1	23	– 5 5	as not to admit of com-
43	9	Gararea	„ 12	„ 6	25	– 6 –	parison from analogy,
44	7	Kahar	„ 14	„ 14	30	– 3 11	or call for further ex-
45	50	Kait	„ 20	„ 15	25	– 7 –	planation at my hands.
46	12	Gararea	„ 21	„ 16	27	– 2 46	The Indian commu-
47	5	Ahier	Aug. 16 '64	Sept. 15 '64	31	– 3 15	nity is divided, as Mr.
48	7	Bhurgee	Sept. 21	Oct. 10	21	– 1 20	Buckle has well ob-
49	7	Chamar	Oct. 12	Nov. 10	29	– – 51	served, into two great
50	30	Kesan	„ 17	„ 16	29	– 2 6	divisions, the very rich
51	7	Ahier	„ 19	„ 11	19	– 6 46	and the very poor—
52	30	Mahajun	„ 19	„ 11	19	– 2 –	there is scarcely an
53	33	Kait	„ 22	„ 10	20	1 5 20	intermediate grade—
54	55	Lohar	Dec. 15	Jan. 15 '65	32	– 6 50	and as the latter pre-
55	30	Mahajun	„ 20	„ 4	15	– 2 3	dominate there as else-
56	3	Mosulman	„ 30	„ 25	26	– – 51	where, so also does
57	25	Mosulman	Jan. 4 '65	Feb. 4	30	1 7 –	stone appear to be, in
58	50	Kesan	Feb. 16	Mar. 14	28	– – 18	greater part, its pecu-
59	9	Bhurgee	„ 19	„ 8	17	– 2 55	liar portion, and for
60	7	Mosulman	April 17	May 10	24	– – 55	the very reasons, as I
61	7	Birhamun	May 3	June 3	31	– 5 10	believe, that scrofula,
62	13	Thakoor	„ 18	„ 10	23	– 3 6	phthisis, and other dis-
63	10	Hache	„ 30	„ 25	26	1 6 49	eases of debility are
64	17	Chamar	July 5	Aug. 6	36	– 6 5	more common among
65	4	Mahajun	„ 5	„ 1	26	– – 46	the poor everywhere
66	10	Chamar	„ 6	„ 4	30	– 6 –	than they are among
67	35	Kurmee	Aug. 5	Sept. 5	30	1 3 51	the rich.
68	9	Ahier	„ 15	„ 15	32	– 2 –	
69	11	Chamar	„ 16	„ 15	31	– 3 10	
70	32	Kesan	„ 28	„ 20	24	– 1 45	
71	20	Kache	Sept. 13	Oct. 10	28	1 1 –	
72	30	Kesan	Oct. 8	„ 30	22	– 5 4	
73	3	Chamar	„ 24	Nov. 20	27	– 1 5	
74	6	Kait	Nov. 5	Dec. 5	30	– 2 51	
75	14	Mosulman	„ 11	„ 5	25	– 5 –	
76	12	Dhanuk	Jan. 28 '66	Feb. 20 '66	24	– 3 40	Of Abyssinian or Ethio-
77	10	Kachee	Feb. 2	„ 28	25	– 1 40	pian origin.
78	6	Kurmee	„ 14	Mar. 14	29	– 6 –	
79	60	Mosulman	Nov. 8	Nov. 30	23	– 2 10	
80	Eight months	Mahajun	Jan. 6 '67	Jan. 30 '67	24	– 1 40	
81	1	Kesan	„ 28	Feb. 20	23	1 – 20	
82	11	Kait	May 7 '69	June 1 '69	24	– 1 52	
83	5	Kesan	„ 12	„ 1	19	– 3 26	
84	32	Brahmin	Oct. 6	Oct. 20	15	2 – –	This man was a prisoner in Futtehgurh jail.

Besides the above lithotomy cases, all of which were successful, the undersigned performed 28 more in 1860 and 1861, while Civil Surgeon of Bijnour. Two of these cases proved fatal, and one of these—the 14th case on which I operated—was a boy about seven or eight years of age, from whose bladder a more than usually large round mulberry calculus was extracted. There was considerable difficulty experienced in extracting it, and in consequence of its shape and size the forceps could not be got to maintain a firm grasp of it. Peritonitis ensued, and death took place on the fifth day. The other fatal case occurred in the person of an old man of broken down constitution, on whom the operation was easily performed, but the wound in the perineum showed no disposition to heal, and he died of exhaustion about ten days after the removal of the stone. Preparatory treatment was not resorted to in any case, and about two-thirds of those subjected to operation at Bijnour were boys under ten years of age. In most cases the stone was extracted the very day on which the patients came to the dispensary. Lateral lithotomy, with an ordinary grooved staff, was invariably practised, and there was seldom any trouble from hæmorrhage. Plugging had to be employed in two instances—both boys—only, and the material used was a flexible tube, a bit of catheter, which was retained in the wound for 24 hours. Cleanliness and a sponge were the only dressings employed, and the result shows that there was no occasion for more complicated manipulation or apparatus.

GEORGE GRANT, M.B.,
Superintending Surgeon Central Prison, Futtehgurh.

Futtehgurh, Nov. 7, 1870.

This confirms what I said above that caste does not materially affect the result or influence the diathesis, as Brahmins and Mosulmen are here as numerous as their neighbours, being seven in each case, and so far in about the proportion they bear to the general community. It also supports the view I have taken of the statistics formerly submitted, and more than sustains the high repute of our Indian brethren, of whose performance the statement of my friend might be taken as a fair echo and illustration. Dr. Grant is not a dashing or brilliant operator; on the contrary, he is slow, cautious, and methodical. But his caution has nothing of timidity in it, his slowness indicates care and a conscientious regard for the responsibilities of his office, and the steady hand, the collected manner, and

the concentrated look bespeak an amount of self-possession that can only be acquired by long practice, and that clearly shows his mastery of the situation. I am proud that it has fallen to my lot to testify thus publicly to the capacity of a friend whose own retiring habits and native modesty would shrink from such a disclosure, whose skill as a physician has been proved in many a rough encounter with tropical disease, and who has achieved a success in a difficult department of his art, in what a great authority would call the "master handywork of surgery," which has not, to my knowledge, been ever exceeded before.

As to the manner of operating, stone is almost invariably extracted in India by the common process of lateral lithotomy, and has not that been sufficiently described elsewhere? I think so certainly, and though I have looked into some books on the subject while preparing this paper, I have no intention of quoting from them. Indeed it was with difficulty I waded through some of them, so finickly minute and circumstantial are they, and so full of details which any man with a diploma ought to be able to supply for himself, and which no man who looks to the success of his treatment could hope to imitate with safety in the hour of trial. Had some of the writers here referred to, witnessed as I have, the facility and readiness with which this process is performed by natives of India who never opened a book on anatomy, and know as much about the Veru Montanum as they do about the man in the moon, they would be less exacting in their requirements, and more disposed to trust to the common-places of surgery and the dictates of common sense. As it is they clearly overdo their parts and invest a simple proceeding with an air of importance and a degree of danger which it does not deserve or entail, and I can scarcely help thinking that the object of such writings is to deter less experienced responsible men from undertaking an operation which is, in my humble opinion, quite within the compass of all. The same remarks will apply to the apparatus employed, and I believe with an old and eminent lithotomist that "with a knife, we have it in our power to make our incisions adequate to the extraction of a stone of any size, and such as will readily admit the forceps, and allow of an easy extraction without laceration. The incision with the knife is at once easy and sure in the hands of one acquainted with the anatomy of the parts; in the hands of one unacquainted with such anatomy, no instrument founded on any principles of mechanism is safe." This ignorance, or inexperience, or whatever

else we may elect to call it—and I have no wish to use stronger language than the occasion will require—is at the bottom of all the strange machinery and complicated apparatus we see figured in books in connexion with this subject, and I believe that a good deal of the mortality that prevails after lithotomy is due to the variegated manœuvring, protracted manipulation, and other rough usage to which unfortunate patients are subjected during its performance. Be this, however, as it may, my experience is entirely in favour of simplicity, and I will now describe a procedure which is practised by simple, uneducated, mountaineer surgeons in the hills, and which, if the information placed at my command respecting it, be reliable —and I have no right or reason to think otherwise—can boast of a success that admits of no approach elsewhere. This is nothing else than the old plan that was practised upwards of two thousand years ago by Ammonius of Alexandria, in the time of Herophilus and Erasistratus, and by Meges, at Rome, in the reign of Augustus, and which is so well described by Allan, Bell, Burns, and Cooper, as to dispense me from the necessity of describing it again. The author of an interesting work, entitled, "A Summer Tour in the Himalayas and Sporting Adventures in the Valley of Cashmere," alluding to the professional performances of the native hakeems or surgeons of the hills, says, at page 212:—"The only surgical operation they perform well is extracting the stone in which they are very successful." So successful, indeed, that, as I was credibly informed by a gentleman who has resided many years among them, and who knows them better, perhaps, than any living European, they lose no more than four per cent. of their cases. Their mode of proceeding may be roughly described as follows:—They begin by thrusting the two first fingers of the left hand as far up the rectum as they can, and placing them behind the stone. As soon as they have found this they drag or push it down till it can be felt in the perineum, and then, but not before, cut upon it with the stump of an old razor, or with the blunt and primitive-looking weapon which is figured on page 329.

They will not operate on particularly obese subjects, or on persons whose redundancy in this respect they cannot easily reduce; but the meagre fare of the hillmen does not often produce embonpoint or require a resort to Banting, and they dispense altogether with the old-fashioned clyster and other preparatory treatment. Should it so happen, however, that a fat patient, who is otherwise fit, presents himself for relief, he is forthwith placed on "short

commons," something like the bread and water diet of our prisons, and thereby soon brought into a condition that will admit of interference. The uncouth instrument represented on the opposite page has been the means of relieving a great many, twenty-seven in all, I believe, and its rude owner was very loath to part with it. It constituted his principal means of support; rough as it was it could not be easily replaced, and it was only by the exercise of a little gentle pressure that he could be brought to terms. At last, yielding to the earnestness of my friend, or "moved by the rhetoric of a silver fee," he consented to part with it, and I now retain it by me as a curiosity. Should the stone refuse to start forward under the *vis a tergo* brought to bear on it from behind, the iron hook or lever, shown in the woodcut, is brought into play, and by means of this, aided by the fingers in the rectum, the offending calculus is dragged *nolens volens* to the front.

But the size or friability of the stone, the depth of the perineum, the restlessness of the patient—for they know nothing of anæsthetics—the escape of fæces from the rectum, or other troublesome occurrence, may prevent or delay the usual happy consummation. In either case the Puharee doctor is equal to the occasion. He is not, as one without resource; on the contrary, he shows himself master of the situation, by thrusting his hand into the recesses of his wallet, and extracting therefrom an implement which is specially kept in reserve for such a contingency. This is nothing else than a pair of pincers, not unlike those used by cobblers for stretching leather, and with this he never fails. The one here figured was used successfully twenty-three times, and was so prized by its owner that I could only obtain permission to sketch it. That was, however, accurately done, and figure No. 3 is a faithful delineation of it.

Such is the apparatus and such the plan of operating practised in the hills, and the outcome of both is a saving of ninety-six per cent., a saving which no other apparatus or operation has ever been able to secure.[a] It is, in fact, unparalleled in the history of lithotomy

[a] I never saw or heard of the high operation or of that through the rectum, in India. I assisted the late Dr. Brinton once during my incumbency as house surgeon at the Royal Free Hospital, Gray's Inn Road, London, in extracting a piece of mortar from the bladder through the rectum of a corpse, but the proceeding struck us both as being little less than barbarous. For instance in which calculi passed or were extracted through the umbilicus and through a fistulous opening in the abdominal walls, see "London Medical Repository, Vol. i., pages 43 and 291."

Fig. 1.

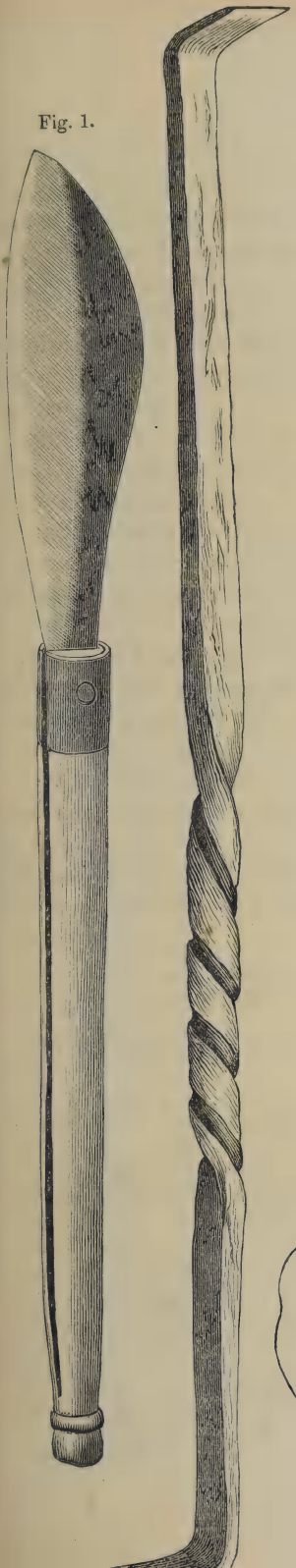
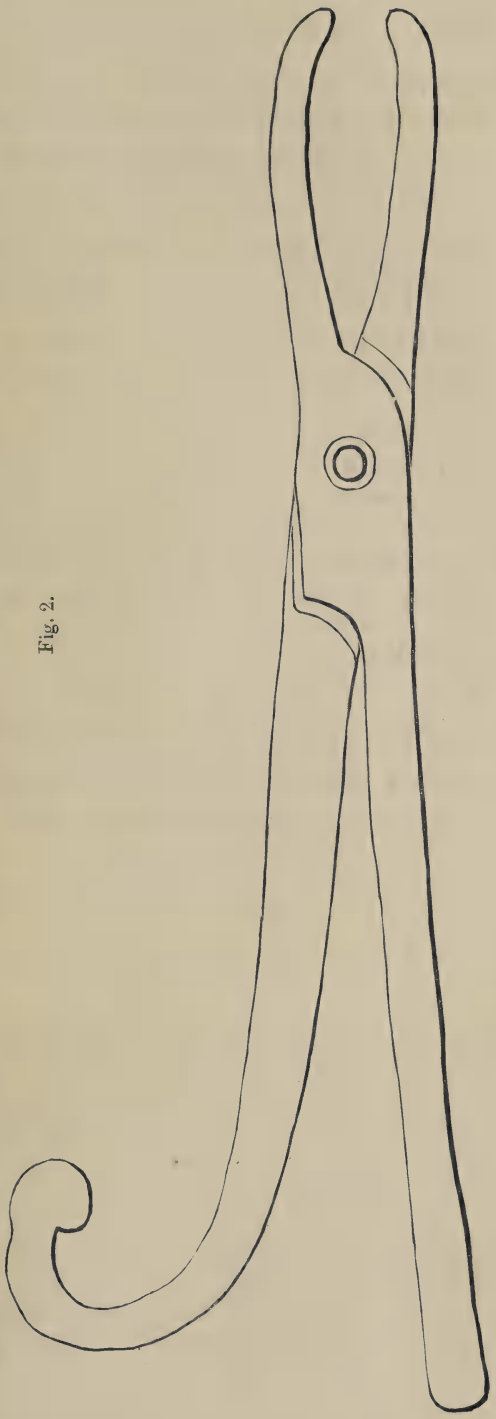


Fig. 2.

Fig. 3.



anywhere, and such a result is calculated to put our boasted superiority to the blush, and prove that nature, however rude and unaided she may be, is a better guide to the complications of her own causing and to the relief of the sufferings she inflicts, than art, with all the resources which a too fastidious and exacting competition may have placed at her command. It also gives additional weight to the old saying, that "there is nothing new under the sun," for the proceeding here referred to is nothing more or less than a modification of the old method of "cutting on the gripe," which was first described by Celsus, and more recently by Allan,[a] Bell, Burns, and Cooper, and which was successfully practised by such surgeons as Hildanus, Heister, Raoux, and others. But to the curious on the subject the authors here quoted are as accessible, perhaps more so, than they are to me, and this part of my paper would be scarcely complete without a reference to the position and practice of the native doctors of the plains. Of these, however, I need not say much as several of them are already well known by their writings or otherwise in Europe, and they are, as a rule, the very antipodes of their rude brethren of the hills. Some of these are doctors of the Calcutta University; some drive large and lucrative practices in the great cities of India; some are professors in provincial schools or personal attendants on native princes; and many are expert and successful lithotomists. But as this paper is likely to assume proportions which I did not contemplate, and as I agree with Horace when he says—

> "Segnius irritant animos demissa per aures,
> Quam quæ sunt oculis subjecta fidelibus."

I will content myself with giving here a representation of an

[a] As Allan's "Treatise on the Operation of Lithotomy" is out of print, and likely to remain so, and as his description of "this antique operation" is the best I am acquainted with, I make no apology for reproducing it in full here. He says, "the manner of operating was this—the rectum was emptied by a glyster a few hours before the operation, the surgeon introduced the fore and middle fingers of his left hand, well oiled, into the anus, while he at the same time pressed with the palm of his right hand on the lower part of the abdomen above the pubis, to assist in bringing down the stone; he then grappled it, brought it forward to press on the neck of the bladder, and made it protrude and form a tumour on the left side of the perineum. He now took the scalpel and made a lunated incision through the skin and cellular substance, directly on the stone, near the anus, down to the neck of the bladder, with horns pointing downwards to the hip. He then made a second incision transversely through the neck of the bladder, and the stone being strongly pressed upon by the fingers, started out into his bosom, or was picked out by a hook provided for the purpose." A Treatise on the Operations of Lithotomy by Robert Allan, surgeon, pages 10–11.

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MALDON, ESSEX

operation by a native surgeon—for which I am indebted to the courtesy of Dr. Playfair, of Agra—and leaving the rest to the indulgent criticism or conjecture of my readers.

From this it will be easy to see who is the master and who is the man. The spectators in the foreground are students, who will in time become dispensers and dressers in Her Majesty's Military Hospitals in the North West, and the expression of all is free from flurry and excitement, and more composed than one would expect under the circumstances, or perhaps than one could find in a similarly constituted assembly at home. And having said so much of the proceedings of others it is now time I should turn to and say something of my own.

My operations extended over a series of years, and were performed in various parts of India, but chiefly in Shahjehanpore, Furruckabad, Cawnpore, Futtehpore, the Punjab and Himalayas. They may, therefore, be fairly said to be representative specimens of their class, and to economize space, I will endeavour to crush into the following table all the facts regarding them that I can now recall, or that appear to me necessary for their clinical history and elucidation. Any that may escape my memory while recording the same, or that may not find room in the table, I will add subsequently, and so bring this already too lengthy communication to a close. And firstly, as regards the operation itself, that, as I said before, was always lateral, and the instruments used in its performance consisted of a scalpel, a staff grooved on the side, or, as I preferred it, down the centre, and the forceps that is always found in the common operating cases furnished by the Government. There was no blunt knife or cutting gorget; no lithotome caché or canule à chemise, nothing, in fact, but the plain apparatus mentioned above, and it accords with my experience that these are quite sufficient for the purposes of lateral lithotomy in nineteen cases out of twenty. In the twentieth case there may be some complication that calls for other instrumental aid or accessory, as happened in an instance mentioned below, wherein I had to use a long bullet forceps, but the most successful lithotomist, will be those who use the simplest instruments, and it will be obviously unnecessary for me, after what has been already said, to prescribe any rules or enter into any details as to the manner of sounding the bladder, holding the staff, or cutting through the perineum. Any man with a diploma ought to be able to effect these objects for himself, and I will never forget the pithy and forcible, but not the less significant and appropriate

advice I received from the gentleman—himself an able lithotomist—who gave me my first case and assisted me in my first operation. While the patient was taking chloroform and I was adjusting my weapons, I asked him if he had any dodge or wrinkle he could put me up to before I began. "No," he said, "get your staff into the bladder and your knife into the groove of the staff, and you must be a greater fool than I take you for if you can't do the rest." And though this direction may not suffice in all cases, it will in most, in proper hands, and it has at least the merit of expressing, in few words, the estimate formed of this operation by practical men in India, and of showing how irrelevant and unnecessary they consider a great deal of what has been written about it at home. The case here referred to was a man in the very prime of life, who recovered without a bad symptom, while that upon which my friend had just operated died from the effects of shock; and this brings me to the reflection that while lithotomy is a comparatively easy proceeding in the adult male, it is quite the reverse in the child, in whose pelvis the viscera are somewhat differently situated, and in whom, to use the words of Mr. Butcher, "the tissues comprising the urethra are so feeble in their cohesion that they will readily yield to violence, and the angle to the bladder is so acute that great facility is afforded to the instrument in going astray." This is strictly true, and equally applicable is his advice about introducing a couple of ounces of water into the bladder before operating, for "it facilitates the detection of the stone, and the stone should invariably be struck previous to cutting the patient—not only because the stone, if a very small one, might have escaped from the bladder, but because this evidence proclaims the presence of the stone, and also the guide to it is direct and certain, the instrument has not gone astray," though I did not always follow it. And apropos of this point and also of the question of lithotomy in children generally, I may here reproduce the substance of a conversation I had with a friend in India, about the time that Mr. now Sir Wm. Fergusson's lectures on the subject were appearing in the *Lancet*, which will better serve to show my estimate of it than any other course or statement I could employ. Having discussed some strange mistakes and malpractices we had witnessed or heard of ourselves, and among others those of tying the urethra in mistake for the perineal artery, cutting through both coats of the bladder into the rectum, and in a third instance including that structure itself, and actually scraping away at the sacrum in lieu of the stone. I asked him, "did you read Fergusson's

lecture about it in last week's *Lancet?*" "Yes," he said, "I did." "Didn't you consider it," I inquired, "very much to the point?" "Yes, yes," he replied, "very much so indeed," and holding up his hands, he added with emphasis, "'tis the very best I ever read on the subject." It is, I subjoined; it deserves to be printed in letters of gold, and "read, learnt, and inwardly digested by every one who undertakes to operate on children and who wishes to do so successfully." And this is, perhaps, as much as I need say on the matter at present.

But it sometimes happened to me, and I know it has often happened to others to have children brought us with all the symptoms of a calculus, in whom, however, none existed, and whose sufferings were induced or aggravated by uncleanly habits, or intestinal irritation. Sounding frequently failed to disprove the fact or throw light on the cause, and it is not to be wondered at that in such instances the usual thing was done. I have heard of several cases in which the operation of opening the bladder was performed, with, of course, negative results as regards the stone, and with no injurious consequences as regards the patient; indeed the proceeding seemed sometimes to have done good by diverting the sufferer's mind from the bladder, or concentrating it on other organs, and inducing his parents to look elsewhere for a remedy. Be that as it may, I never heard of a death from such manipulation, but the mistake must necessarily be an unpleasant one for a sensitive mind, and I was happily saved from resorting to it myself, by remembering an anecdote of the late Mr. Liston, whose authority on this and other similar details could admit of no question. It is told of that distinguished surgeon, that, while lecturing one day on the symptoms of stone in the child, he had exhausted the usual stock somewhat sooner than he anticipated, and was obliged to fall back on his own experience for illustration. He accordingly addressed his class as follows:—"But, gentlemen, when you happen to be in doubt about your case, I advise you to place him on a chair and desire him to jump down; if he has no stone he'll readily do so, but if he has, he'll see you d—ned first." And this is a test which can be so readily resorted to by all, that it deserves more attention than it would appear to have received. I acted on it in a case of the kind here contemplated, and finding that my little patient readily complied with my request, I refused to operate, and I was afterwards glad I did so, for his worst symptoms yielded to time and treatment, and he ultimately regained perfect control of his water. As

intimated before, I do not pretend to entire accuracy in the particulars given below; it is always difficult to obtain reliable information from the natives, and this difficulty was enhanced in my case. One daily meets very old men in India, but their ages can, in many instances, only be inferred from their appearances, and, when interrogated on the point, they either refer back to some great event, such as the siege of Bhurtpore, the battles of Lord Lake, or the reign of Runjeet Sing, which may have happened in the time of their grandfathers, or they put you off with a shake of the head and a Khuda Jane—God knows.[a] The same may be said, to some extent, of the account they give of the rise and duration of their symptoms, for with them time is of no value, they act on the Epicurean maxim of caring for nothing but their bellies, food and farthings engross all their thoughts, and I never yet heard the poorer class of natives discussing anything else than the price of grain, or the fluctuation of wages.[b]

[a] I find the same trait or peculiarity ascribed to the Indians of South America, by the great traveller, Humboldt. He says, "travellers who merely judge from the physiognomy of the Indians are tempted to believe that it is rare to see old men amongst them. In fact, without consulting parish registers, which in warm climates are devoured by the ants every twenty or thirty years, it is very difficult to form any idea of the age of Indians. They themselves are completely ignorant of it." "Life and Travels of Alexander Von Humboldt." New York: Rudd & Carleton.

[b] How often have the words of Horace occurred to me when listening to or looking at a group of natives:—

"Nil admirari, prope resestuna Numici,
Solaque quæ possit facere et servare beatum."

As regards the size and composition of calculi I made the following note, which is, perhaps, worth preserving. "In the Futtegurh Dispensary, there were on the 13th of October, 1866, eighty-six calculi, all the subjects of which had recovered. They appeared to consist chiefly of uric acid and phosphate of lime. I noticed more than one of a dark brown colour. A large, egg-shaped, seemingly phosphatic stone weighed 7 drachms and 40 grains. Another of a similar description weighed 1 ounce and 15 grains, while a third brownish specimen, the neck of which appeared to have been adjusted to the prostatic portion of the urethra, weighed 7 drachms and 19 grains. There was one stone—the subject of which, who was over sixty, had died—kept apart, which appeared rough and jointed like a piece of consolidated lime from an old house, and which weighed 3 ounces 4 drachms and 5 grains. My own specimens have lost considerably in weight since they were extracted. It stated in an old number of the *Lancet*, that Mr. Paget removed a calculus at St. Bartholomew's that weighed upwards of 9 ounces. Mr. De Morgan mentions a case in the Pathological Transactions of London, Vol. xxi., p. 271, in which two calculi, which weighed together 22½ ounces, were removed from the body of a man who had exhibited symptoms of stone for years, but who was supposed to be shamming for the purpose of obtaining poor-law relief. In the same volume there is a woodcut of a stone which weighs 25 ounces, and which was taken from the body of one Sir Thomas Adams, who accompanied General Monk to Buda to congratulate and bring the King—Charles—home.

Name and Age	Caste	General Health	Previous duration of urinary symptoms	Other complications	Operation	Date	Calculus					Time under treatment	Result	Remarks
							Size	Weight			Probable chemical composition			
								oz	drs.	sc.	grs.			
1 Dulhu, 40	Kurmie, Villager	Fair	8 months	None	Lateral	June 4, 1862	Pigeon's egg	--	6	1	5	Calcic oxalate	16 days	Good	Lived on grain or dhal—a species of grain or pulse whereon cows, horses, sheep, and the like are occasionally fed—Indian corn and green vegetables, in other words, his sustenance differed but litttle from that of the beasts among whom he dwelt; I did not ascertain whether there was any hereditary tendency in his case. He recovered well and rapidly, while the man who was operated on by another and more experienced surgeon at the same time, and with similar instruments, died a few hours afterwards of the effects of shock; urine passed through urethra on the 6th day after operation.
2 Lullee, 12	Behana	Below par	4 years	Debility	Lateral	June 11, 1862	Four-sided pippin	—	6	—	—	Lithic (uric) acid	17 days	Satisfactory	Recovery was retarded by an attack of sub-acute peritonitis, and he was discharged for change, with the perineal wound still unclosed. He passed water, however, through the urethra, and I subsequently ascertained that the wound cicatrized well, and that he regained the full use of his bladder.
3 Kunghun, 35	Cachee	Good	1 year	None	Lateral	July 15, 1862	Triangle-shaped and irregular	2	1	1	16	Calcic oxalate	28 days	Good	It is quite impossible to ascertain in many, if not in most cases, whether there has or has not been any hereditary predisposition to stone, but I infer there was such in this instance from his surroundings and other sources, and he recovered well. The urethral passage was restored on the 1st of August, and when last seen he was quite free from symptoms of urinary irritation. This was the only one of my cases that contained three calculi, and these exhibit regular facets, the result, doubtless, of intra-vesical friction.
4 Kusala, 12	Kerane or Cultivator	Fair	1 year	None	Lateral	Sept. 2, 1862	Large pigeon's egg	—	6	—	—	Lithic acid and lithate of amminum	26 days	Good	An old man who was operated on the same morning by another officer, and whose deep perineum offered considerable obstruction to the extraction of the stone, died of shock soon afterwards, while my case, though the operation was equally protracted through the friability of the calculus, recovered well. The urethral passage was restored on the 8th day after extraction.
5 Kishna, 55	Brahmin Priest	Aged and indifferent, with flabby muscular development and no fat	2 years	Premature old age	Lateral	March 18, 1863	Like a cone tree fruit, and as large as a hen's egg	1	7	—	—	Calcic oxalate	13 days	Death	A bad case, *ab initio*, as his health was completely undermined by long suffering, and he came from the hills of Nepaul. The climate of the plains was therefore unsuited to him, and diarrhœa having set in he died of exhaustion on the 13th day of his admission. The perineal wound had nearly closed. The coats of the bladder felt tough and corrigated; the muscles of the belly were shrunken and atrophied, and he looked altogether a much older man in death than he appeared to be during life.

Name and Age	Caste	General Health	Previous duration of urinary symptoms	Other complications	Operation	Date	Calculus					Time under treatment	Result	Remarks
							Size	Weight			Probable chemical composition			
								oz.	drs.	sc. grs.				
Missree, 15	Brahmin	Good	3 years or upwards	None	Lateral	May 3, 1863	Filbert	—	—	12 —	Calcic oxalate	31 days	Good	Two small stones, the smaller of the two weighing only a few grains, were extracted in the usual way, and with no other difficulty than what occurred in catching the small one, which had to be fixed with the finger, and then removed with a dressing forceps.
Beedee, 35	Hillman	Indifferent	several years	Appears to have lost all control over his bladder	Lateral	Nov., 1863	Turkey's egg	2	6	— —	Calcic oxalate	27 days	Good	Recovered without a bad symptom, and the wound was quite healed ere he left.
Radha, 14	—	Good	18 months	None	Lateral	Dec. 3, 1863	Bantam egg	—	5	1 15	Lithic acid and calcic oxalate	11 days	Good	Was taken away by his friends, and lost sight of a few days after the operation, but it was subsequently discovered that he recovered well, and was able to resume his ordinary duties.
Ishmael, 35	Mahomedan	Good	Uncertain, but believed to be upwards of 4 years	None	Lateral	Aug. 8, 1864	—	—	2	1 —	Calcic and magnesic phosphates	2 days	Good	The stone, which appeared to consist of mixed phosphates, and was very soft and friable, crumpled to pieces in the grasp of the forceps. The debris was washed out by syringing, and the whole were found to weigh two drachms one scruple and fifteen grains. The patient recovered well, and the urethral passage was quite restored on the 22nd of August.
Sheo Narain, 10	Cultivator	Good	About 2 years	None	Lateral	Oct. 1, 1864	Small hen egg	1	1	— 5	Lithic acid	17 days	Good	An ordinary case that calls for no special record.
Bahadur, 5	Bunya (trader)	Good	18 months	None	Lateral	Aug. 5, 1865	Rough and nodulated	—	1	— 4	Calcic oxalate	15 days	Good	Do., and the urine was flowing freely through the urethra within seven days.
Gunga, about 45	Boatman	Fair, but somewhat impaired by long suffering	About 7 years, as far as could be ascertained	Cannot retain his water long, and his clothes and person stink horribly	Lateral	Sept. 4, 1865	Turkey's egg	3	2	— —	Calcic oxalate	36 days	Satisfactory	Some difficulty was experienced in grasping the stone, which appeared to fill the whole bladder, and which, moreover, being placed transversely across the perineal opening had to be turned upon its own axis, and so withdrawn. He was much depressed by the shock of the operation and the chloroform, but soon rallied, and his recovery though slow was sure. The urine kept dribbling through the wound longer in this instance than in any other of my recoveries, but it created no irritation.
Fagoo, 14	Cow-herd	Good	One year and a half	None	Lateral	Jan. 28, 1866	Large pigeon's egg	—	4	— 15	Earthy phosphates	15 days	Good	Recovered without a single bad symptom, and was passing water per urethram on the 5th day.
Meena, 31	Chumar	Indifferent	3 years	—	Lateral	May 7, 1866	Large walnut	½	—	1 —	—	See remarks	Good	This man bolted at 2 a.m. on the morning of the 2nd day after operation from hospital, and returned to his home in a village on the Oude side of the Ganges. The urine came through the urethra from the very beginning, and when seen by me some 8 or 10 days later the wound in the perineum had nearly closed, and this too in face of his having gone about his

Hulloo or Kulloo, 63	Mahomedan	Indifferent	15 years	A feeble old man, who has evidently suffered long and severely, and who looks a bad subject for such an operation	Lateral	May 11, 1866	Plover's egg	—	3½	—	—	Calcic oxalate	43 days	Recovery	He recovered well notwithstanding his great age and long suffering, and when taking leave of me, before setting out for his home, he wanted to kiss my feet, and when prevented from doing so he salaamed me with a suppleness and activity which clearly showed that he had taken a new lease of life. I subsequently learned that he took unto himself a new wife, but whether or how long he survived that encounter I cannot undertake to say.
Heera, 5	Cultivator	Good	2 years	Is liable to frequent attacks of diarrhœa	Lateral	May 14, 1866	Two stones one the size of a plum stone and the other like a cherry	—	1	—	15	Calcic oxalate	33 days	See Remarks	On first attempting to remove these calculi I failed to find them, though I followed the usual course, and fancied I entered the bladder. Unwilling to endanger the child's life by what then appeared a fruitless search, I sent him back to bed, and exactly a week afterwards reopened the wound, and after a good deal of patient search and careful fingering succeeded in entering the bladder, and removing therefrom the two stones here described.
Krisn, 11	Melon producer	Good	3 years	None	Lateral	May 27, 1866	A flattened pebble	—	—	2	16	Calcic oxalate	13 days	Good	Passed water within a day or so of the operation through the urethra, and the sore was quite cicatrized on the 10th day after extraction.
Muthra Pershad, 32	Coolie	Good	6 years	Looks considerably older than he really is	Lateral	June 5, 1866	Oblong, and pear-shaped	1	2	—	5	—	25 days	Good	I had some difficulty in removing this stone in consequence of having commenced my incision too high up in the perineum, and not extending it back enough into the bladder. I had accordingly to enlarge the wound with the right hand, while I held its lips open with the blades of the forceps, grasping the stone in the left, and I effected both without any hæmorrhage or laceration.
Chitha, 5	Grain seller	Good	15 months	Rather small and feeble for his years	Lateral	June 8, 1866	Filbert and walnut	—	1	2	—	Lithic acid and dodic l thate	22 days	Good	I experienced great and unusual difficulty in the removal of these stones, for the bladder was so high up in the pelvis, so remotely situated therein, and withal so small, that it was both difficult to find it in the first place, and when found it was not easy to enter it or seize the stone. The curve of the staff was besides so closely grasped by tough unyielding membrane that it was very difficult to keep the finger from slipping away from it. Knowing, however, that any such slip or deviation might involve the death of my patient I stuck to it like wax, and after much wriggling and some unavoidable laceration succeeded in reaching the little cavity, and removing its troublesome contents. A fistulous opening which remained in the urethra was subsequently rectified by perineal section.
Tha Koor, 25	Tha Koor (high caste Hindoo)	Good	3½ years	None	Lateral	June 22, 1866	Plum-shaped and very friable	—	1	—	10	Triple phosphates	41 days	Good	There is no other history than that he recovered well, and was soon afterwards able to resume, without suffering, his ordinary duties.

Name and Age	Caste	General Health	Previous duration of urinary symptoms	Other complications	Operation	Date	Calculus					Time under treatment	Result	Remarks
							Size	Weight oz.	drs.	sc.	grs.	Probable chemical composition			
Bala Ram, over 40	Katee or cultivator	Indifferent	Probably 11 years	Has lost flesh and strength of late, and his wife says he once attempted self-destruction	Lateral	June 28, 1866	Small hen's egg	2	2	—	5		35 days	Recovery	The bladder did not regain its tone in this instance as soon as in the other cases, there was, however, no other untoward symptom, and the case was complete before he left. He was a person of very feeble intellect and limited understanding, and the bodily confirmation appeared to partake of the defects of both.
Gopal, 55	Grasscutter	Failing	As long as he can remember, but never suffered much	Has incipient cataract in left eye	Lateral	July 7, 1866	Turkey's egg	2	2	—	—	Calcic and magnesic phosphates	43 days	Recovery	The recovery was slow as might be expected from his old age and long suffering, and the bladder did not regain entire control of its contents for several months subsequently.
Ram Lall, 13	Mahajun banker	Good	2 years	None	Lateral	Aug. 17, 1866	Nutmeg	—	1	—	5	Lithic acid	21 days	Recovery	Except that the chloroform made a greater impression, and produced more sickness in his case than in that of any native I have yet seen, there was nothing to record or complain of.
Gosain, 35	Kait cultivator	Fair, but he looks thin and worn	5 years or upwards	Cant retain his urine, the penis is unusually long and flabby, and his clothes stink badly	Lateral	Sept. 15, 1866	Large pigeon's egg, smooth and round	—	4	—	15	Calcic oxalate	3 weeks	Recovery	Recovered slowly but satisfactorily.
Mahadeo, 3	—	Good	11 months	None	Lateral	Oct. 18, 1866	Flattened cherry	—	1	—	16	Lithic acid, traces of oxolates	—	Recovery	For this case, which I saw in private in the house of a Hindoo tradesman, or rather usurer, of the Bunya caste, I received a fee of rupees fifty, which was paid down on the nail before I left my bungalow. I only saw the child once afterwards, and he was not subjected to restraint at any time.
Moormee, 7	Kesan, farmer	Good	About 2 years or upwards	Was very irritable from suffering, and did not take kindly to his food	Lateral	Nov. 15, 1866	Filbert	—	1	—	8	Calcic oxalate	27 days	Recovery	Was carried away by his parents on the 2nd night after operation, who brought him back, however, soon afterwards, on being threatened with punishment by me. The change did not in any way injure him. Two calculi came away, one of which was a mere pebble.
Dunga, 27	Chumar, shoemaker	Good	3½ years	Has frequent attacks of strangury, and occasional paroxysms of acute pain	Lateral	Dec. 22, 1866	Pigeon's egg, rough and corrugated	—	4	1	—	Calcic oxalate	39 days	Recovery	Though he got quite stout on the diet allowed him before and after operation, he yet occasionally passed blood in the urine, and there were other appearances which led me to suspect the presence of renal calculus. He left, however, greatly relieved.

alla Pershad,12	Bunya, grain seller, &c.	Good	2 years	Dyspepsia	Lateral	May, 1867	Small bantam egg, round and rough	—	2	2	—	—	—	Recovery	This was one of my casual cases, that is cases picked up by myself, and though the patient suffered from an attack of hæmorrhage, which was left, unknown to me, to right itself, and subsequently from fever, he yet recovered well and obtained full control over his bladder.
oorga, 27	Fruit seller and Coolie	Good	5 years	None	Lateral	June or July, 1867	Hen's egg	2	1	—	—	Calcic and magnesic phosphates	3 weeks	Recovery	I had to notch the neck of the bladder on either side, and enlarge the orifice of the perineal wound laterally before I could extract this stone. There was some bleeding in consequence, which, however, was easily arrested by cold douching, and he recovered completely.
meer Jan, 17	Mahomedan and Coolie	Good	Uncertain	None	Lateral	Sept., 1867.	Plover's egg, round and smooth	—	3½	—	—	Calcic oxalate	—	Recovery	I cut this man in the hills, on my way to Cashmere, in September, 1867, and he was quite well on my return towards the end of October. I had no other assistance than what I obtained from my own servants. I left him two hours after the operation, after sharing with him the contents of my brandy flask, and simply desiring him to douche the wound daily, and turn over on his belly when he wanted to make water. He probably did not attend to either of these directions, and yet got quite well all the same.
holab Shah, 70	Mahomedan	Indifferent	8 years	See Remarks	Lateral	June 27, 1869	Broke up into seven different fragments	1	4	1	10	Calcic oxalate	17 days	Death	A man, aged about 70, in the very last stage of decrepitude and debility, who has suffered for at least 8 years from symptoms of stone, was brought to me by his son, to Camp Koongagullee, near Murree, on the 6th of June, 1869, and after being detained for the purpose of enabling me to procure more suitable instruments than I possessed, and also in view to my effecting some improvement in his general health, was operated on on the 27th of same month, in presence of Drs. Atkinson and Morgan, of H. M.'s 19th and 6th Regiments respectively. The stone broke down under the grasp of the forceps, and had to be removed piecemeal, and the patient was so enfeebled by suffering, and so debilitated by shock. &c., that I had some difficulty in bringing him round. He recovered, however, under the influence of beef-tea and champagne, and urine escaped through the natural passage two days afterwards. Fever supervened about the 4th of July, and continued to increase in spite of quinine, arsenic, and support of all kinds. The stomach became soon afterwards irritable and intolerant of food, and he passed away quietly about 12½ a.m. on the morning of July 14th. I ascertained, by digital touch, that very little union had taken place in the wound; and I feared from the beginning that he would not have stamina enough to enable it to do so.

This table presents few points of divergence from those already given; in all the results are pretty much the same, and it will be seen from mine and Dr. Grant's that the operation was eminently successful in children. But this is so much the case elsewhere also, that I am inclined to look to the operator for any departure from it, and I cannot too strongly impress on the minds of my younger brethren the necessity of forgetting for the time or ignoring altogether what they may have learnt of the position of the bladder in the adult, when they come to deal with the subject of calculous disorder in children. The relative positions are almost reversed, the one being low down towards or rather upon the rectum, and the other above tilted forwards under the pubis; but they'll learn these things better from Sir Wm. Fergusson's excellent lectures than they can from me, and with regard to their incisions they cannot do better than act on the advice of Mr. Butcher. That able operator writing on this point in the *Dublin Quarterly Journal*, for February, 1870, says:—"I cannot lay too much stress on the necessity of freely opening the entire of the membranous portion of the urethra, by the one and continuous stroke of the knife. This should be the surgeon's aim, for if the knife be introduced again and again with the intention of clearing the staff, the difficulties of completing the operation are greatly increased, the urethra is wounded and notched in several parts. Shreds of it may hang into the groove of the staff, and it may be so impaired that the remaining connecting tissues may fail to resist the efforts essential to the completion of the operation and give way, and so the surgeon may be foiled in reaching the bladder." This, however, ought not to happen as long as the staff is adhered to; *that should on no account be lost sight of*, like the poor compassless benighted mariner described by the poet, who looked to one solitary star alone for the guidance of his struggling ship, the operator should say, "if I lose thee I'm lost," and he who sticks to the staff, will in all probability reach the stone and save his patient. No matter how awkward and bungling may be his manipulation, no matter how vague and imperfect his knowledge of anatomy, no matter how feeble and faulty his incisions, provided, of course, they are not carried too far, if he sticks to the staff and *that that is in the bladder and on the stone*, he ought, if he be not a fool, or something worse, to feel and remove the latter. Sir Henry Thompson says, that "the most frequent cause of death after lithotomy in children is peritonitis and constitutional exhaustion," and that is doubtless so in England; but the deaths I witnessed or

heard of among children in India, appeared to me rather to be due to shock, hæmorrhage, or injury of the rectum, and I did not find that a few years one way or the other materially affected the result. Advanced age, on the contrary, does so terribly, by preventing union of the perineal wound, inducing irritative fever, diminishing the desire for food, and the capacity for sleep, and ultimately paving the way for sub-acute peritonitis or incurable exhaustion.[a] My two fatal cases were both old and feeble, and the last was an old man who had to be carried by his son, and who could not, in any case, long survive his sufferings. Yet he recovered well from the effects of the chloroform and the shock of the operation, but the wound in the bladder refused to heal; he lost his appetite, and could not sleep; apthæ appeared on the tongue; and he passed away at last, without pain.

For the analyses of the calculi given in the Table, I am indebted to the courtesy of Dr. Cameron, the accomplished Chemist and Analyst to the City of Dublin, who most kindly undertook the labour of determining for me the composition of the stones which I had brought home.

And thus I bring to a close my personal experience of lithotomy in India.

ART. XXI.—*On Death Registration and "Medical Certificate of Death."* Read at Medical Society of the College of Physicians, Ireland, 15th March, 1871, by SIR DOMINIC CORRIGAN, Bart.

MY object in asking your attention to the present system of Death Registration and "Medical Certificate of Death," is with the hope that in the coming legislation on sanitary measures the defects now existing may be corrected.

[a] "Barnes, the celebrated editor of the *Times*, having suffered for years from stone, was at length persuaded to submit to an operation, which, although it was most skilfully performed by Liston, gave such a shock to his nervous system that he sunk under it, and died on the 7th of May, 1841, in his fifty-sixth year."—Andrew's History of British Journalism, Vol. ii., p. 84.

"Suffering for years" is a bad preparation for such an operation, and shock to the nervous system is much more likely to occur and prove fatal after the powers of life are on the wane than before. For other instances of great men in whom delay proved dangerous, see the preface, &c., to Mr. Allarton's "Median Lithotomy," and Traver's "Constitutional Irritation."

The following is the form of "Medical Certificate of Death" under the Irish Act of 26 Vict., cap. 11, required to be filled up by the medical attendant of a deceased person:—

"To the Registrar of the District of , in the Union of
"in the County of .
"I hereby certify that I attended ; who was apparently
"aged, or was stated to be aged years; that I last saw him on
" day of , 18 ; that he died on day of ,
"at ; that the cause of death was ; and that the
"disease had continued
"Witness my hand this day of
"Signature
"Profession
"Residence ."

The following is the section of the same Act under which the above form is required to be filled up—sec. 46:—

"Whereas . . . be it therefore enacted . . . the medical
"practitioner who shall have been in attendance during the last illness
"and until the death of any person dying after the thirty-first day of
"December, One Thousand Eight Hundred and Sixty-three, shall within
"*seven days* after the death of such person transmit to the registrar of the
"district in which the death occurred, a certificate of the cause of death
"in the form mentioned, the particulars of which shall be entered by the
"registrar in the register. In case such certificate shall not be trans-
"mitted, the registrar shall transmit to such medical practitioner a form
"of such certificate and by a written or printed requisition under his
"hand shall require such medical practitioner *forthwith* to return to him
"such certificate duly filled up, and said medical practitioner shall *within*
"*three days* after the receipt thereof, return said certificate duly filled up
"to such registrar."

This Act was passed on the 20th April, 1863. I gave it on its appearance my attentive consideration. I was most anxious, as, indeed, all members of our profession ever have been, to give every assistance to the promotion of all measures tending to the benefit of the community; but, after full consideration, I came to the conclusion that few cases, if any, could ever occur in which I should feel myself warranted in filling up the "Medical Certificate of Death" as required by the Act of Parliament.

I thought the fair and straightforward course to adopt, and I did adopt it soon after the passing of the Act, was to wait upon some of the official gentlemen appointed to carry out the Act, and

acquaint them with my views, and my determination that I would not, because I could not, in truth, fill the certificate required.

It was then explained to me that the English Act left it a voluntary matter with the English medical practitioner to give the required medical certificate, but as that plan did not work, the Scottish Act of 1854, imposed a penalty of forty shillings on the medical practitioner who omitted to transmit the required certificate, and as that course too failed, the pecuniary penalty was omitted in the Irish Act in order to substitute a more severe punishment, that of misdemeanour for disobeying an Act of Parliament. This intimation was not calculated to make me more yielding, and I have refused all requisitions to fill up and sign the required certificates.

I shall now state some of the grounds on which I have felt I could not in truth fill up and sign the "Medical Certificate of Death" as required by Act of Parliament.

The words of the Act are, "that the medical practitioner who shall have been in attendance until the death" is to fill up and sign a certificate "in the form mentioned." No man can be considered as "in attendance until the death" unless he was *present at the death*. The interval of an hour equally with that of a month intervening between his last visit and "the death" puts him out of the category of being "in attendance until death."

I now turn to the certificate itself. It requires the medical practitioner to "*certify*" to three things, viz.: first, the day of death; second, the cause of death; third, the length of time the disease had continued. The phrase "certify" applied to facts means that the party "certifying" possesses and gives "certain" information of the facts so certified. Facts cannot be certified on hearsay evidence. Can the medical practitioner in most cases "certify" of his own personal knowledge, the day of death, the cause of death, and the duration of the disease? In eight years that have elapsed since the passing of the Act only one instance occurred in which I could "certify," that is, of my own personal knowledge, state that death did occur on a particular day. Can the medical practitioner in the great majority of instances, "certify" as to the duration of disease?

A man may be for years affected with disease, with cirrhosis of the lung or liver, with disease of the heart or great vessels, with paralysis or other derangements of health, and may continue to live until some complication is added, and under such circumstances what is the disease meant in the certificate? Is it the disease that

has existed for years, or is it the incident that has immediately preceded death? On this the Act of Parliament and form of certificate are equally silent.

As to the duration of disease—information on this head, in the great majority of cases, must be given altogether on hearsay evidence. All these points so different and so necessary to be kept distinct, are jumbled together in the certificate.

I may now mention some circumstances which it appears should make us very cautious as to certifying facts without personal knowledge of them.

I was in attendance on a patient who was labouring under fatal disease. On the occasion of a visit I was informed by a person who met me on my entrance that the patient was dead. Another person more distant beckoned to me to go up stairs. I found visitors in the room transacting business with the patient. I might have been seriously and properly compromised if I had filled a certificate on the statement made to me.

Another case came within my knowledge. A very heavy annual insurance was payable. Payment was delayed in the expectation that death would take place within the time allowed for payment of the insurance. Payment and death were pitted against one another; and the medical practitioner, trusting altogether to a hearsay statement, signed a certificate that death had occurred on the last day for paying the premium.

Perhaps few practitioners think of what they are doing in signing "certificates of death" on hearsay evidence, and fewer still of the heavy liabilities they may incur.

Dr. Lankester, Coroner for Middlesex, states, in one of his reports, that in England "medical certificates have been registered in cases where the medical practitioner has not seen the patient alive, and in some cases where he had neither seen him alive nor dead;" and Dr. Rumsey, in his essay "On State Medicine in Great Britain and Ireland," gives this warning:—"That a certificate given incautiously for a reported death may render the signer liable to be indicted for a conspiracy to extort money on false pretences from an insurance office."

Communications have been repeatedly addressed to me, and I think to many around me, requesting that we should certify as to "date of death," "cause of death," and "duration of disease," in cases where not only days, but weeks, and in some instances months, have elapsed after we had seen the cases.

If any one among us were asked to certify under his hand that the Holyhead mail had sailed this morning from Kingstown he would not do it, although he had far more credible and reliable evidence of it than he could have of the facts the Act of Parliament requires him to certify in the medical certificate. We should not, I think, do that in our professional capacity which we would not do on our personal responsibility.

In the old coroners' law of England, the certificate of death was required to be given only *super visum corporis*. That was as it should be.

In France the law as regards registries of deaths is that the relatives or inmates of the house where a death occurs send notice to the district officer, analogous to our coroner. He visits the house and certifies to the identity and death, and to the date on the testimony of those around the deceased, and then obtains from the medical attendant of the deceased a certified statement of such particulars as came within his personal knowledge, and no more.

It is, I believe, only by the introduction of some plan similar to the French system that we can arrive at truth as to the registry of disease and death.

We have at this moment three different laws as to registration of deaths in England, Scotland, and Ireland.

In England the filling of a certificate of disease and death is optional with the medical practitioner. The result is as stated by Dr. Rumsey, already quoted in his essay on State medicine, that in England we are far from possessing any trustworthy records.

In Scotland there is a penalty of forty shillings on the medical practitioner who does not fill up the required certificate. The penalty could not be enforced, for a man cannot be compelled to certify what he is not certain of; and so in Scotland the Act is a failure. Dr. Christison, in the proceedings of the Social Science Association at Edinburgh, stated that one-fourth of the mortuary returns of Scotland were useless; and Mr. Walker, at a meeting of the Glasgow Medical Association, 1866, informs the meeting that the mortuary returns of Scotland were not worth the paper they were written on.

In Ireland a more severe penalty is threatened by the Act of 26 Vic., c. 11, viz.: the penalty of misdemeanour for not filling the certificate of death, but no official has ever ventured to bring a recusant into Court, and I think for this good reason, that no Court would inflict a penalty of misdemeanour on a man for not

certifying what he could not certify. What, then, is our relation to the present form of registration of deaths, and what should be our course?

Our present relation to the registry of deaths is that we are required by Act of Parliament to certify under our hand what we do not know to be facts; and I think there cannot be a question that we ought not to do that.

The next question is, what should be our course and what is our duty to truth and to ourselves? If we are not warranted, in the great majority of cases, in signing the form of medical certificate of death as laid down in the Act of Parliament, our clear and truthful course is to abstain from signing it, and only sign it in the rare instances in which we can conscientiously do it.

Let the Legislature mend its ways and correct the present defective state of the law without expecting the medical profession to sacrifice truth and self-respect to prop blundering Acts of Parliament.

I hope this state of things will soon be amended. A revised code of sanitary laws is almost now under consideration, and I trust our Legislators will learn that in framing such laws they should seek for assistance from those bodies that understand the subject.

ART. XXII.—*On the Modern Aspects of Therapeutics.*[a] By WALTER G. SMITH, M.D., Dubl. Univ.; Fellow and Censor King and Queen's College of Physicians; Assistant Physician to the Adelaide Hospital.

IT must be allowed that the reproaches which have been so often levelled against the *practice* of medicine have had much foundation in the past history of therapeutics, and all will re-echo Sir T. Watson's opinion, that "certainly, the greatest gap in the science of medicine is to be found in its final and supreme stage—the stage of therapeutics." Some of its keenest satirists have been physicians of the highest eminence and most varied acquirements, and, on the whole, it must be admitted, that the improvement of therapeutics, contrary to the other arts and sciences, "bears no proportion to its antiquity." It is the consciousness of this disproportion which damps the spirits of so many in the profession, and which has led to so much distrust and doubt. Dr. Radcliffe used to say that "the whole art of physic, for which he had a profound contempt, might

[a] Thesis for the degree of M.D., 1870. Read before the Medical Society of the College of Physicians, March, 1871.

be written on one sheet of paper," and it is not so long since the late Sir W. Hamilton, of Edinburgh, asked the question:—"Has the *practice* of medicine made a single step since Hippocrates?" a revival of the older query, *an datur ars medicinæ?*

There are many evidences that the need for a more careful study of therapeutics is urgently felt by the body of the profession at large. In 1865, the Physiological sub-section of the Brit. Med. Assoc. drew up a memorial to the Gen. Med. Council praying the council "by pecuniary grants, and the appointment of suitable persons, to undertake investigations into the physiological action of medicines." This memorial was supported in the council by the Regius Professors of Physic in the Universities of Dublin and Oxford, but was negatived on the ground of want of powers in the council to comply with the petition.

A sub-committee was then appointed by the Brit. Med. Assoc. and the results of its labours are seen in the elaborate report brought out by Dr. Hughes Bennett, on the action of mercury, podophyllin, and taraxacum on the biliary secretion. About the same time the Royal Med. Chir. Soc. intrusted the examination of the method of subcutaneous injection to a committee, and the valuable observations embodied in their report furnish the most satisfactory data which we possess respecting this method. Quite recently the Med. Psychol. Assoc. of Edinburgh, have appointed a committee for the purpose of taking into consideration, among other things, the medical treatment of insanity, and they suggest propositions for combined therapeutical investigation, and ask for special information on the action of chloral. The Clinical Society of London owes its establishment in 1868, to the expressed want of more real knowledge on the various remedies in daily use, and the appearance of numerous detached papers, and of some works of merit on the doctrines and requirements of therapeutics testify to the deep-seated interest which now attaches to the prosecution of this subject.

I propose, now, briefly to inquire what are the resources at our command, and how far it may be said that therapeutics has advanced within the last quarter of a century, what are the hindrances to its progress, and, more particularly, in what directions we may hope for still further and more solid advances than have yet been gained.[a]

To avoid entering upon too wide a field my observations will be

[a] For many suggestions I am especially indebted to, and have largely made use of Sir W. Jenner's admirable address on medicine, delivered last year, in Leeds, and Dr. Rogers' recent able work on therapeutics.

chiefly confined to the domain of what may be called medicinal therapeutics, *i.e.*, of remedial agents as directly applied to the treatment of disease, and accordingly the steady progress and increased knowledge of sanitary science and preventive medicine, the splendid results of operative surgery, and the development of state medicine, will be passed over without comment.

The retrospect of the history of therapeutics for centuries past, is, in many respects, not encouraging, and one can scarcely help wishing that much, if not most, of what is called the accumulated experience of ages were swept clean out of remembrance, so overladen is it with confusion, mis-statements, and unproven theories. In fact since the prevailing ideas as to the action of drugs became in some degree fixed at a time when pathology was less exact than it is now, when there were no such accurate means of testing the real effects of remedies, and when physics and chemistry were in their infancy, we cannot avoid insisting on the necessity for renewed observations, carried out under better auspices, and with a better directed aim.

Yet it will be conceded that the materia medica abounds in agents by means of which very remarkable effects can be produced on the human frame, and a speculative mind might engage itself in showing that the possession of such powers by various medicines is an argument in favour of our being intended to exercise a due control over the progress of disease. Even as it is we can, at will, exalt or depress the action of the heart, the great fountain of life, and can, to some extent, control the capillary circulation, we can compel the stomach to eject its contents, and the intestines to discharge their excreta. We have agents that act on special functions of the encephalon, on the spinal cord, on the sensitive nerves, and purely on the motor nerves. By suitable means we can increase or diminish the exhalation from the skin and mucous membranes, and can alter in quality and quantity the secretions of many important glandular organs. At pleasure we can contract or dilate the pupil of the eye, can stimulate striped and unstriped muscles, can poison some internal parasites with certainty, and can aid in the elimination of metallic poisons from the body. And, let it be observed, that not only have we these and other powerful means at our disposal, but that, in many, very many cases, we have the knowledge *how* to apply them to the treatment of disease with benefits which cannot be gainsaid, and in a few cases we know *why* we so apply them.

Our theories as to the nature of disease are undergoing a profound change, necessarily followed by corresponding modifications in the way in which we endeavour to meet or anticipate it. The notions of elimination, and allopathy, of antidotes, and of counter-irritation, have all their measure of truth, and are all usefully applied in practice, but it is to be hoped that none of them will ever again be raised to the rank of a system to cramp and fetter our ideas. As a positive and well-founded advance in the doctrines of therapeutics, it could easily be shown that certain injudicious or noxious lines of treatment have been abandoned, and that, in general, the habit of over-drugging has been given up. This beneficial change is due partly to a more accurate acquaintance with the local causes of disease, *e.g.*, the parasitic skin diseases, partly to a more intimate knowledge of the pathology of disease, *e.g.*, chronic pulmonary phthisis, and partly to a recognition of the principle that we are not to treat our patients as so many sponges doomed to soak up the maximum quantity of medicine possible, but, as living beings, whose functions are disordered by disease, and whom we seek to restore to health by aiding the natural tendency to recover, and by striving to modify the direction of action of the natural forces of the body. We know now that a large number of acute diseases occurring in previously healthy persons naturally run a definite course and tend to spontaneous recovery, in the absence of or even in spite of misdirected drugging, and we have recognized that certain acute diseases supposed to be of indefinite duration lie within appointed limits. We, therefore, by this advance in knowledge, avoid drawing false conclusions as to the efficacy of drugs in particular maladies, and although we do not pretend to be able to strangle acute disease by specifics, or suddenly arrest the cycle of morbid action, much still remains for our art in meeting special symptoms and controlling intercurrent complications. Sometimes advances in knowledge teach us a more correct appreciation of the composition and mode of action of drugs, or at least displace a faulty explanation. This certainly is a gain, and we know too little yet to see how far the application of the physical processes, dialysis, diffusion, and osmosis may before long enlighten some of the dark recesses of therapeutics.

Among the tributes levied from chemistry and natural history, we can reckon carbolic acid and its compounds, the alkaloids, the bromides, permanganate of potassium, sulphurous acid and the sulphites, the whole group of anæsthetics, chloroform, ether, bichloride of methylene, nitrous oxide, and nitrite of amyl, Calabar

bean, glycerin, pepsin, santonin, podophyllum, and lastly chloral, and its allies bromal and iodal. The mention of the class of alkaloids suggests the thought that very great benefit would, doubtless, accrue from the more extended use of the alkaloids in the room of the crude vegetable products from which they are derived. Our therapeutical experience would be rendered infinitely more accurate by the employment of these definite active principles which are chemically stable, and whose dosage can be exactly proportioned, and the differences which are often asserted to exist between the active principle and the crude drug itself would doubtless be found to be much less considerable than is generally thought. In the case of belladonna and conium, for example, the efficacy of these drugs is fairly and fully represented by their respective alkaloids, and even in the case of a complex substance like opium which contains several organic bases of different properties, it would be quite possible, after proper investigations, to combine these bases in a compound solution so as to represent perfectly the action of the crude opium. As illustrations of the confirmation and extension of the curative powers of single drugs we can adduce the mass of evidence that now exists as to the respective value of mercury and iodide of potassium in different stages of syphilis, and of mercury especially in infantile syphilis, of the utility of arsenic in the *relapsing* skin diseases, of bromide of potassium in epilepsy, and certain other abnormal conditions of the brain and sexual organs, of quinine in periodic diseases other than ague, and of ipecacuanha in dysentery. We are better acquainted with the action of digitalis, opium, belladonna, hyoscyamus, and conium, and there is a clearer understanding gaining ground as to the worth and indications for the employment of alcohol in the treatment of disease.

The uses of iodide of potassium have been brought into greater prominence and have been more sharply defined, and amongst the results "we may boast the disappearance of radesyge in Norway, of yaws in our West Indian colonies, and of most of the severe forms of tertiary syphilis at home." Since the more important of these drugs are of quite recent introduction, they are to be looked on as but an earnest of the harvest we are yet to reap from the domain of the natural sciences. Improved modes of administration are only second in importance, and hypodermic injection is an aid for which we cannot be too grateful, triumphing especially in the relief of painful and spasmodic affections. Lastly, a discrimination between the properties and uses of the direct and induced currents, *i.e.*, of

galvanization and faradization, has led to most important and gratifying results in the treatment of such formidable diseases as epileptiform neuralgia, infantile paralysis, and progressive muscular atrophy. It is proved that it is possible and feasible to galvanize directly the brain and spinal cord, and the galvanic irritation of the sympathetic nerve may yet furnish us with a powerful lever for controlling the nutrition of even remote parts.

Many circumstances have contributed to clog the progress of therapeutics, some of which belong to the inherent difficulties of its investigations, while others, and that a large portion, are due to the ignorance and incompetence of those to whom we should look for aid. The fallacies connected with the application of the inductive method of reasoning to the science of medicine, and the sources of error in practical and theoretical medicine, have been well exposed by Sir G. Blane and by Dr. Barclay, and I would merely remark that the principles enunciated by these authors, while they are the philosophical basis of the practice of physic, constitute the best answer to morbid scepticism on the one hand and vulgar credulity on the other.

Faulty modes of preparation, and the use of entirely worthless compounds, are fruitful sources of error, and we can point in illustration to the investigations of Dr. Harley on the galenical preparations of conium, in which he proves the absolute valuelessness of the extractum conii. Again, the assemblage of a number of active drugs in a prescription, often introduced at random, is destructive to a right appreciation of the effects of medicines, and, as a rule, the principle of combination should not be extensively tried till we are in a better position to estimate justly the influence of certain drugs on special diseases.

It has lately become the fashion to decry the study of materia medica, and it is asserted that the possession of such knowledge is a useless burden on the memory. I am persuaded that this is a mistake, and a serious one, and I am sure that many will from repeated experience bear me out in the belief that an accurate knowledge of the characters and properties of drugs is of every day utility to the prescriber, in enabling him to formulate correctly, to detect imposture, to avoid improper combinations, and to explain any phenomena that may unexpectedly arise.

Since our ignorance of the curative resources of the organism, and of the healing powers of drugs have been, and still are, the chief sources of error in therapeutics, and the chief obstacles to its

improvement, it follows that the foundation stone for positive knowledge must be laid in more accurate investigations into the real properties of drugs, and this leads me to consider how we may best set about such improvement, and in what directions we can look for assistance in such a course. I shall pass over without further reference the direct gains to therapeutics, and the lessening of the chances of confusion which flow from improved methods of diagnosis, from the more strict localization and classification of disease, and from the prosecution of physiological and pathological studies, and will direct attention, in the first place, to the influence which organic chemistry and physics are now extending over practical medicine.

The outcome of all recent developments in science, and in especial, the doctrine of the correlation of force, *i.e.*, the indestructibility or conservation of energy, the corner-stone of science, has been to render it in the highest degree probable that plants and animals are under the operation of the same laws as inorganic nature, and that all the changes and processes which are unceasingly at work within us are mainly the result of the action of physical and chemical forces upon the material constituents of our frame. The human body has often been resembled to a machine, and though the comparison between a living body and an inanimate machine should not be pushed too far, still the forces operating on each can reasonably be compared, and the more closely we know the limits of health, and the deviations that may occur from it consistent with life, the more surely can we propose to rectify the errors in function. Hence it is plain that a truly expressed science of medicine cannot be evolved except by endeavouring to refer the processes going on in the animal body, and therefore also the influence of remedies on these, to the ultimate laws of physics, chemistry, and physiology. "Chemical inquiry is now finding its way into many of the remoter secrets of function, and is likely before long to establish some laws of molecular constitution which will enable us to classify unknown remedies, and to explain and calculate their actions."—(Dr. Allbutt.)

The observations of Bence Jones and Dupré, who were the pioneers of this work in this country, have disclosed a rich mine of discovery, and they have demonstrated the existence of a chemical circulation within the body, which rivals in importance that of the older mechanical circulation of the blood. By the application of spectrum analysis they have shown the wonderful rapidity with which crystalloids diffuse from the blood into the colloid tissues, and from the tissues into the absorbents, and so the passage of all

substances through the human body is determined by the laws of diffusion, modified by pressure. For example, if 20 grs. of carbonate of lithium are taken into the stomach, it will in two and a half hours have passed into every particle of the textures, and beyond the blood circulation even into the most distant parts, and in three and a half hours it will be distinctly present in each particle of the lens. In about seven days the lithium will be entirely eliminated from the body. When 7 grs. of carbonate of lithium were given eight hours before delivery, the lithium was subsequently detected in each particle of the umbilical cord.

Again, they have determined the existence, in animals, of a widely diffused substance which closely resembles quinine, and which has been named animal quinoidine. This leads to a plausible supposition, the only one yet offered, as to the mode of action of quinine in curing ague, and the hypothesis, though not proven, opens up a hopeful prospect of possible discovery.

The history of organic synthesis dates only from the year 1828, and remained comparatively barren for some years, but since the year 1845, its progress has been truly marvellous. The most complex substances are being formed at will, while the last barriers between organic and inorganic bodies are disappearing, and as the advances in this branch of science are, if I may say so, in the highest degree cumulative, the time is probably not far distant when, by the artificial formation of morphia and quinia, we shall be able to dispense with the production of opium, and the cultivation of cinchona in our colonies.

Every schoolboy is now familiar with the derivation of the most diverse colours from coal tar, and it is but the other day that alizarine, the colouring principle of madder, has been built up from another component of coal tar—the first instance of the artificial production of a vegetable colouring matter. We have just learned that artificial indigo has been isolated, and we may confidently hope soon to see the alkaloids brought into the market, derived not from their natural sources, and dependent on precarious supplies, but furnished to us by the laboratory of the chemist—the true magician of our age. [Even since these lines have been written, Schiff has announced the first attainment of this result in the artificial formation of conia.] The insight which we will thus gain into the constitution and intimate nature of complex organic molecules must prove of inestimable value as a stepping-stone to a true classification of remedies. So comprehensive is the aim of modern

chemistry, and so wide the means of research, that "we can foresee a state of chemistry in which, without studying the properties of different bodies in detail, and knowing only the number, atomicity, and electric polarity of the elements, it will be possible to determine by simple calculation the formulæ, properties, and mode of preparation of all compounds possible" (Naquet.)

In a philosophic and suggestive paper, Dr. Broadbent has made a bold attempt to apply chemical principles in explanation of the action of remedies and poisons, in which are contained, I believe, the elementary principles of scientific therapeutics. Starting from the two postulates—1st. That there must be some relation between the substance administered and the human organism on which the effects produced depend. 2nd. That, so far as the substance is concerned, the basis of the relation can only be its *chemical* properties, using this term in its widest sense, certain important corollaries flow from these:—1. That the physiological and therapeutical actions of the same substance must be similar in kind. 2. That the action of foods, medicaments, and poisons, in the system, must be capable of explanation on the same principle. 3. That substances closely allied chemically, must have an analogous action on the system, or the diversity in their operations should be capable of explanation on chemical principles; in other words, chemical groups ought to form therapeutical groups. This is an outline of the path to be pursued, and some steps of importance have been already gained by individual workers. In England and Scotland the names of Bence Jones, Richardson, Crum Brown, and Fraser, stand out in honourable relief; in France, among a number of observers, Mialhe, Rabuteau, and MM. Pélissard, Jolyet, and Cahours; and in Germany, Liebreich, Binz, and many others, have pursued the investigation of the physical and chemical action of drugs with results most encouraging, though, as yet, imperfect and incomplete.

In determining the action of any substance from a chemical point of view, Dr. Richardson has shown that we have to consider five points, viz.:—1. The elementary basic or radical composition of the substance to be tested, and the changes of constitution to which it may be subjected; 2. The physical qualities of the substance; 3. The chemical stability of the substance; 4. The physical peculiarities of the animal body subjected to the substance; and 5. The special action of the substance on special centres of the animal organism.

Some scattered attempts to express the relation which, no doubt,

exists between the physiological action of a substance and its chemical composition and constitution (*i.e.*, the mutual relation of the atoms in the compound) have from time to time been made, but until lately with trifling success. For example, it has long been observed that, as a rule, the salts of the same base and of the same acid have respectively a common physiological action, and Mr. Blake, of California, pointed out many years ago, and has lately extended his experiments, that, in general, isomorphous substances have analogous actions.

But the most decided step in this direction has been made by Drs. Crum Brown and Fraser, in their important papers on *the Connexion between Chemical Constitution and Physiological Action*, (1868-69). By introducing a known chemical change into the constitution of a physiologically active substance, without breaking up its molecule, they have shown that the physiological action of the substance may be completely altered, and, in fact, inverted in kind.

They have examined with great care the physiological action of the salts of the ammonium-bases derived from eight of the better known alkaloids, and their results lead to the suspicion that chemical *condensation* (*i.e.*, susceptibility of addition) is in some way connected with physiological activity, and that saturated bodies (*i.e.*, whose condensation = °) are inert or nearly so. Thus by the addition of iodide of methyl to the non-saturated base strychnia, the poisonous activity of that alkaloid is diminished at least 210 times, and a quantity of iodide of methyl-strychnium, containing 21 grs. of strychnia, can be given to a rabbit with impunity. These observations are of the highest value, though at present they must be considered as but foretastes of what is to come, and it is remarkable that almost immediately after, two French physiologists, MM. Jolyet and Cahours published results corresponding in almost every respect with those of Brown and Fraser.

Dr. Richardson has done good work in the field of anæsthetics in their chemico-physical relations, and he has brought out the curious and interesting fact that, in the alcohol group, the anæsthetic effect has a definite connexion with the chemical composition of the alcohol, the anæsthesia rising in proportion to the number of atoms of carbon; for example, contrast the action of ethylic alcohol, containing C_2, with amylic alcohol, containing C_5. It is observed also that definite changes are produced by the addition or substitution of new elements or radicals, such as H, Cl, I, C_2H_5, &c., and when the chemical relationships between different bodies are more

thoroughly understood, we may eventually be able to deduce *à priori* the physiological action of a body from its known chemical history.

Dr. Rabuteau, who has made many contributions to physiological chemistry, believes that he is justified by his investigations in propounding, as a general law, that "the metals are more active physiologically, according as their atomic weights are more elevated, or, what is the same thing, as their specific heats are lower," *e.g.*, Na, K, and Tl. The diatomic metalloids conform also to this atomic law, but the monads, curiously, are governed by a law which is the reverse of this. Thus F, Cl, Br, and I, is the order of physiological activity of the halogens, and this is precisely the inverse order of their affinity for O.

These illustrations are, at least, sufficient to shadow forth the assistance, qualitative and quantitative, which we may expect from physical and chemical science, and warrant us in believing in a sure foundation for future therapeutics. It is true that the facts, as yet known, are mostly isolated and disconnected, but we may compare them to separate bricks which, though singly of little value and without cohesion, yet when cemented and fitted together, will form a firm and durable superstructure. The physiological school, headed by C. Bernard and Brown-Séquard, has done much to elucidate the action of some most important drugs, and it is likely that the doctrine of physiological antagonists will lead to practical results.

The different effects of remedies when introduced by different channels, the principle of the administration of smaller doses frequently repeated, and the potency of drugs over the vaso-motor nerves are all receiving a greater or less share of attention, and are exerting a wholesome influence on our habits and methods of prescribing.

Yet even with the most perfect knowledge of the chemical and other properties of drugs, we cannot satisfactorily judge of the influence which they exert on disease, unless we know, in any case of recovery in which medicine has been used, what share is to be assigned to the curative power of the organism itself. The evident importance of this inquiry was recognized by the Austrian School of Medicine for years before it attracted much attention in these countries, and we have now, at all events, learned that a large proportion of diseases, numbering some of the most formidable character, may get well without the use of any drugs whatsoever, or, in other words, they have a natural tendency to terminate in the

restoration of health. This salutary change of doctrine is due in part to an examination of the undeniable results afforded by homœopathic practice, but largely owes its impetus to the improved state of physiological and pathological science. There is, however, some danger of being over-zealous in our respect for nature's operations, for the efforts of nature are not always of a benignant tendency, and what is called "expectant medicine" may sometimes prove but "a meditation upon death."

A more accurate knowledge of the real properties of drugs than we have hitherto possessed, lies at the root of all future progress, and the mode of its accomplishment claims attention at the outset. This will be best carried out by carefully conducted trials on *healthy* individuals, checked by collateral experiments on the lower animals, and on patients suffering from diseases whose diagnosis, general course, and variations, are tolerably well known. Hitherto it has been almost exclusively the custom to endeavour to acquire a knowledge of medicines by instituting trials with them in disease, a method which has borne little fruit in return for the labour bestowed upon it. To Hahnemann, in particular, before he was carried away by the delusion of infinitesimal doses, belongs the credit of actively pushing forward the proving of medicines on healthy individuals, recommended by Störck, Alexander, and Haller, and it is strange that, with very few exceptions, no provings of worth have been made by other practitioners until very lately.

Within the last two years Dr. J. Harley has shown the value of this line of inquiry in his elaborate and searching work on the action of opium, belladonna, conium, and hyoscyamus, in which he has done much towards defining our knowledge of the effects and uses of these ancient neurotic remedies.

One most important issue of the careful testing of drugs would be the better determination of the "sphere of action" of each medicine, for it is already well known that certain drugs affect particular organs and tissues, and I believe, with Dr. Rogers, that this significant fact of drugs possessing elective affinities for certain textures will occupy a prominent place in our future therapeutics. We have reason to believe that the physiological and therapeutical actions of medicines are very closely related, and it is probable that the modifications impressed by various diseased conditions will not so materially alter their sphere of action, as is sometimes supposed.

Another real gain from this probation of drugs would be the

expulsion from the Materia Medica of a crowd of articles which only serve to keep alive the embers of polypharmacy, and to obstruct our advance towards a more rational system of therapeutics. If we accept, as we may safely do, the axiom that a drug, which produces no perceptible effects when properly tested on healthy individuals, will prove equally inert in disease; what a host of reputed medicines would be cast into deserved oblivion?

Before concluding, I wish to point out most emphatically that we should not allow ourselves to overlook the continued necessity for bedside observation in our admiration of the progress and prospects of the scientific departments of medicine. Though our theoretical knowledge were ever so perfect, yet clinical experience must always hold an important position to every true physician, and "it is to the experience of the mass of the profession that we look for the final establishment of doctrine and rules of practice." The most rapid and complete advances in science can never do away with the necessity for watchful observation, and "the nice adaptation of means to end can only be gained by experience." The past history of medicine should teach us not to be too hasty in condemning or ridiculing a line of practice which united and prolonged experience has approved, even if it be contrary to the received dogmas of the day, or be incapable of immediate explanation. Rational experience must and will keep its place. Let it by all means be reinforced and directed aright, but not trammelled, and clinical researches and empirical decisions must eventually prove the touchstone of therapeutical theory.

Keeping in view, then, that the three chief aims and objects of medicine, especially so far as concerns the non-professional public, ought to be the cure of disease, the prolongation of life, and the alleviation of physical suffering, we can sum up, in Sir W. Jenner's words, our gains in practical medicine as resulting in "advances in knowledge, in the addition to the science of medicine of new facts, the elimination of supposed facts, the more correct appreciation of the bearing of old facts, and the application of this new knowledge to the advancement of the practical objects of the science."

And, though the discoveries of our own time naturally appear to us of greater importance than those of preceding ages, even the most incredulous will admit that we have reached a stage when ignorance is giving way to knowledge, hypothesis to facts, and that the time is approaching when we shall be able to free ourselves from the quicksands of uncertainty, and rest on the firm basis of knowledge and truth.

ART. XXIII.—*The Present State of Therapeutics.* By S. M. HEWITT, L.K.Q.C.P., L.R.C.S.; Physician to City of Dublin Hospital; Physician to the Dublin Dispensary for Skin Diseases, and Demonstrator of Anatomy, Royal College of Surgeons.

THE application to clinical work of the various instruments now used, viz., thermometer, laryngoscope, sphygmograph, ophthalmoscope, &c., and the efforts of thoughtful men have done much to place our recognition of disease on a sound basis. Physiology in its later advances has at length in many points reduced to physical and chemical laws the so-called vital phenomena of secretion, nutrition, heat, &c. Pathology, aided by the microscope, has also endeavoured successfully to keep pace with the general movement forward, we hear less of "specific" products—we are often able completely to bridge the chasm which separates the knowledge derived from the *post-mortem* room from that obtained at the bed-side, and though the lifeless mass to the uninstructed, presents but a sorry picture of the "myriad" processes of vital activity and gives only gloomy prospects of our power over disease, yet lighted by our recollections of organs seen in the full vigour of their work, and by the experience of how many symptoms of disease must as yet retain their rank of functional derangements, we at the present moment stand in a more perfect condition with regard to disease in its nature, pathology, and diagnosis than has been possible for any past generation to do. In marked contrast to this is our position with regard to practical therapeutics. It is notorious that many of our greatest workers in the fields of physiology, pathology, and diagnosis are almost entirely sceptical as to the action of drugs. We are here divided into hostile camps—expectant, restorative, heroic, antiphlogistic, are well-known epithets of contending factions—who are all united on one point only—the absolute worthlessness of any treatment but their own. Treatments, the most various, are supported by lists of cases cured by the "method," until the inexperienced is utterly bewildered which of the many recommendations to adopt. It cannot be considered an idle task at the present time to inquire briefly into the causes of this chaos of opinions and to endeavour to elucidate in what direction real advances may be looked for.

Those who have paid the closest attention to the actual cure of

disease are in my experience the most hopeful of its possibility, and no one can long be an hospital physician without learning, at least, that we have the power of checking symptoms and alleviating suffering, if as yet pathological processes are, for the most part, removed from under our control. It is not then in any unfriendly spirit that I wish to criticize our present knowledge, it is rather with a feeling of great hopefulness for the future, for I cannot understand if the body is obnoxious to injurious chemical and physical agencies which produce its diseases, why the same organism should be removed from the action of other agencies which conserve its powers and act as antagonists to the destructive ones. The previous failures in this field are to be attributed to the great difficulty of the subject rather than to any proved or even generally assumed impossibilty of it, and to the methods employed; which I hope to show could never by themselves yield satisfactory results in a large proportion of cases.

How then, it may be inquired, have we arrived at the present unsettled state of opinion as to the value of treatment in disease?

1stly—Because in many cases where our forefathers fancied they wrought great cures we have been shown that a method of treatment whose glory it is to do nothing can give results which, to say the least, are not in any way less favourable than those before obtained. This discovery that disease, even severe disease, has a tendency to run a definite course to returning health, which may be proximately stated to be (inversely as the deviation) without any aid whatever, has given a shock to results formerly obtained which in a great measure deprives them of utility.

2ndly—Because increased diagnostic power has separated diseases before considered as examples of the same disorder, placing one in a category that gets well without treatment and the other among those that must progress, thus often destroying the credit given to drugs of curing a formidable disorder by showing that an insufficient diagnosis was the real cause of the supposed cure.

3rdly—By trying a remedy upon hopelessly advanced cases of disease, in which it is really efficacious only in the earlier stages, and by the use of doses unsuitable either by their insufficiency or excess, forgetting two great truths, that the same drug has different actions in different doses, and in different conditions of the same individual.

4thly—By the unlimited application of statistics to these inquiries; but more of this hereafter; enough has been said to

show that the present state of scepticism was inevitably brought upon us by our very advances showing the slender foundation upon which many remedies were administered, and in many cases the absolute uselessness of our medication.

It will now be well to glance at the various steps which have gradually led up to our present practice, and to compare their applicability with those which more modern medicine suggests.

The most primitive method of therapeutics derived from the time when diseases were considered to be evil spirits rioting in the body, which required simply to be expelled, has proved also the most lasting; it was to take a person suffering from a certain disorder and subject him to experiments with different substances until we found one to check or control the severity of the symptoms, then arguing that every one affected by the same entity would be relieved by the same remedy, it became established as a cure for the disorder, completely irrespective of the "patient." Thus the great doctrine of empiric medicine took its rise. A system which at one time occupied a sovereign position in therapeutics, and which, if you would estimate what its results were in the zenith of its power you must read the medical literature of the seventeenth and eighteenth centuries; while its present bearings can only fairly be obtained by remembering that it is at once the great discoverer of quinine, and its influences over ague; ipecacuanha, and its action in dysentery; while at the same time it is the great originator of unguents and plasters, which, under concealed names, are the great bane of the unenlightened public, and which are placed by them on a level with the results obtained by the constant careful study of disease at the hands of an educated man. It is no wonder that at the present day there is an intense longing to get from under its control. It seems like the last sighings of an ancient spirit breathing a different language and uttering thoughts at strife with all the advancement that surrounds it.

The next step of any importance which was taken in advance was the administration of the same remedies for diseases whose symptoms were analogous or whose supposed nature was the same, and although this principle often failed, through a misconception of the real relations between diseases having similar symptoms, yet it introduced into medicine the first step from the simplest forms of experience.

In this way, metallic salts were transferred from one convulsive disorder to another, until we find them recommended in all forms

of tremor, convulsion, chorea, epilepsy, and complaints too numerous to mention; and tobacco from strychnia poisoning to tetanus, spasmodic stricture, strangulated hernia, and vaginismus; but it is impossible to pursue this method of treating analogous symptoms by the same remedies very long without introducing absolute chaos into our work, since diseases so absolutely different so often present symptoms similar in nearly every respect.

The last great principle which was enunciated by those who clung rigorously to the empiric method was the acknowledgment that local lesions were frequently the result of constitutional disorders, and to be treated mainly with reference to the state of the system generally. I need not dwell on how much this great discovery has in modern times been extended so as to make medicine ridiculous and to bar our progress to anything like accuracy. We have now so many diatheses that the local treatment of skin affections is often laughed at as sheer nonsense, and we are told to attack the constitution as the only resource, and patients are dosed with arsenic, &c. If we can make out that an old person has at any time of life had gout and is now suffering from bronchitis it is put down as caused by the "poison;" the same may be said for diarrhœa, and we hear from practitioners of a great many nameless disorders, neuralgias, &c., from the same cause.

These three principles then, of empiric remedies for disease; of analogous diseases, curable by the same remedies; and of the constitutional treatment of local disorders may be taken as the ancient contribution to a System of Therapeutics.

And in dismissing them for the present we cannot help being struck with the fertility of the imagination and the stedfastness of purpose with which every country, animal, and plant was ransacked to obtain materials for the support of this gigantic empiricism; nor can we avoid being oppressed with the feeling of awe at the enormous load of superstition which has been swept away by increasing knowledge of disease.

I regret to have to record that what has been done in modern times for actual practical medicine has in it so much of a destructive as opposed to a constructive character; but it was inevitable that it should be so, and it is clearly our interest as well as our privilege to endeavour to hand down to posterity the field cleared, at least, from weeds, if we cannot plant in it any great additions.

Now, in the first place, as long as the natural course of disease

was unknown the effects of remedies could only be very loosely tested, and though numbers of cures could always be cited in favour of any method, it could never be ascertained how much the remedy did for the disease, except by introducing statistics on a larger scale than can usually be employed in these inquiries, for no succession of equally severe cases ever occurs in a series of cycles prepared by Providence to favour the advance of medical science, and if we compare two groups of cases bearing the same specific name—one group by the general type being mild gives admirable results, the other treated under different circumstances by the same remedies brings out diametrically opposed results, so that after all the great want in our inquiries has been some common standard, not in the name of the disease only, but in the severity of the general group of symptoms. To take an illustration. It is now well known that many cases of pneumonia will run a course of about a week to returning health without interference of any kind whatever, but that there are others which have an inevitable tendency to end fatally, while a third set can never at any moment be said to have a distinct tendency in either direction. Now, it is plain that in order to make any legitimate comparison of these cases it is not only necessary to classify them as pneumonias, but to discover a group of symptoms which will mark the cases with an unfavourable tendency, and that remedies be applied for experimental purposes only in those that have this group of symptoms. In accepting the statements of foreign authors as to drugs, this is especially necessary. It will be in the recollection of all the extravagant praise bestowed on camphor in the late epidemic of cholera from abroad; and in the various reports the favourableness of the results were just measured by the number of cases of diarrhœa included.

Statistics without some such rule as that pointed out have proved a worse than useless ally to medicine. I have heard in the College of Physicians Hall a dozen successive speakers handle the same figures to prove diametrically opposite results, and this will ever be the case until we arrive at some common standard from which to view our cases.

Again, it is ever being forgotten that a disorder at one time "curable" becomes, of necessity, in its progress, utterly beyond the imaginable control of *art*, it may be that a pneumonia is curable, but who will undertake the removal of a *caseous* degeneration of the same; or we may be able to prevent the accession of

endocarditis in rheumatic fever, but who will remove the fibrinous thickening and contraction of the valves which is its result. Yet we often in practice deal as if remedies for commencing phthisis were as applicable at the end, or as if we had the most certain methods of arresting cerebral hæmorrhage or meningitis, the result of tubercle.

It has been found that as we abandon the doctrine of specific diseases, so in the treatment, the importance of the malady retires and the patient becomes prominent. Thus we have no treatment for *any disease* which is universally applicable, we may *use* mercury to-day and stimulants to-morrow; we may treat an adult one way and a child another; we have nothing that can in any sense be considered a universal antidote for a disease, and we cannot tell what ought to be done by hearing its name. Perhaps little need be said on this subject here, but it was only a few weeks ago that Dr. Wilks, of Guy's Hospital, after pointing out many abuses arising out of prevailing scepticism on the actions of remedies, tells us that he thinks that we ought to treat only the disorder, give what experience tells us is best for its cure, and leave considerations about the individual out of account. No doubt the modern method has had many abuses; we exaggerate the difficulty of removing symptoms by a simple line of treatment; we may too often neglect the use of certain remedies for fear of their effects on the strength of the patient, but I fancy the tide of opinion will not be easily drifted back into the atmosphere of specific remedies because certain individuals refuse their assent to well ascertained rules of treatment.

Having then paid due attention to the natural course of the disease, and at the same time to the individual under treatment, the first great task before us is to ascertain what the actual value of old methods of treatment is, where there is either a tendency towards death or at least grave doubts which way the case will terminate. This I should say can only be done by statistical results, and in applying the experiment there are two rules which ought at all events to be followed, first—to give *one drug only*, and secondly, to give it in doses which have some effect which can be ascertained. When looking over a book of prescriptions, such as Beasley's collection, taken from all the great practitioners of France and Great Britain, it is surprising the enormous length some of these reach to; and how many incompatibles they contain; now, it is but a clumsy way of doing work after all, if we must

combine a number of medicines to act on a disease when we don't know which is the right one. Again, it is mostly impossible to find out the dose of a preparation that is to produce an effect, we are often guided in practice by the thought if I give such a large one it will sicken, or give headache, or depress, when, if we were acting with confidence in the remedy, we would give it boldly to work what we expect, confident that the inconvenience would be felt only for a time. The smallness of modern doses is, I fear, often the result of inward scepticism, as to the usefulness of the remedy; and it certainly has very frequently this effect, that it deprives medicines which have a real curative power in large quantities of any effect whatever in the case. It is too much the fashion to believe that if large doses have a marked effect small ones will have a sure but insensible one in the same direction. Now Dr. Harley has shown that small doses of conium have no action whatever on the nervous system, while large ones have well marked effects; and it has long been ascertained that the same substance will have results completely contrary to one another according to the quantity taken. For example, ether, alcohol, and chloroform are narcotic poisons in large quantities, but restoratives in small ones; ipecacuanha, an emetic in one dose, while it acts as an anti-emetic in another.

The modification of the ancient method of discovering a remedy which may be applied successfully to a large number of cases of disease that modern ideas seems to require, is, that it shall have been applied in cases with a known termination when unaided, and that it shall have a proved result upon that ending, it is impossible for long centuries to hope that the empirical method of treatment can be altogether abandoned, since we know as yet so little of either disease or health in their mutual connexions. What we must do is to guard it from the abuses it has up to the present suffered, by more rigorous testing of results, at the same time supplementing it by that, which to us educated in modern times offers the only final goal that we can hope our art will reach, namely, the discovery of the action of each body on the physiological functions, and the application of these substances to perform their known effects—to restore those functions when depressed or disordered.

We do not any longer take the most nauseous substances or those most repugnant to our natural tastes and administer them in every disease to try and find among them all, one that will do good, but taking any chemical compound whose effects are unknown we aim

at finding out what its exact effect is upon a healthy individual, and if we gain any results, use it in diseased states, confident that in all but exceptional cases its action will be the same.

This importance of the physiological action of drugs is one of the most peculiar elements of modern medicine, and though it has *as yet* done little to add to our actual practical store of remedial agencies, yet on all hands it gives rich promise that it will not long remain unfruitful, and it has corrected errors which had lain for ages undiscovered *without* its help.

Historically speaking, it was the great founder of homœopathy which first suggested and carried out this research into the action of medicines on the healthy state, obviously for very different purposes from what we would have, and the results have been so vitiated by his peculiar tenets as to be almost worthless. The first necessity that lies upon us then, is to prove the action of each medicine on a series of healthy individuals large enough to give general results. This can only be done in laboratories set apart for the purpose in connexion with our schools, and must be tried on animals and verified on man. It is much to be regretted that in the education of students, so large a part of his knowledge of drugs, their preparations and combinations, should be relegated to an apothecary's shop, or left altogether uncultivated. This subject of *materia medica and therapeutics* is divided for him into many parts—a little taught at hospitals, some in lectures, and after all no practical acquaintance with the real action of medicines on the healthy or with their taste or preparations is given to him at all. It is a general complaint that no student can prescribe, but I do not wonder that this is the case when the information is attained in such a scattered manner; so much do I know this to be the case, that I would suggest an easy alteration in the working of the schools which would make a vast difference in the amount of knowledge that a student could attain, and give that practical character to the information which is alone useful.

The great study which occupies the attention of students for their three years after they leave hospital each day is anatomy; the reason is plain enough, they can work at it themselves, it is a practical tangible subject in which they have a direct duty to perform, and a distinct set of facts to be observed. Now, I do not wish to underrate the value of anatomical work; but this monopoly is, I believe, entirely injurious, and ought to be done away with. This can be done by the establishment of a Demonstratorship of Materia

Medica with a simple laboratory, which men should attend in their turn, and where they might see the action of medicines tested physiologically on animals, no matter how few, and where they might become, under suitable guidance, acquainted with the effects of important drugs on their own person, a kind of knowledge most valuable in the treatment of disease in others. I am sure that I am right in stating that thus educated, we would become practitioners with a much larger acquaintance with the action of drugs than anything we can at present boast of; while of all places such a laboratory would be the fruitful source of discoveries of new substances with distinct physiological effects. It would be a noble contribution from the Irish schools if in the next twenty years we could furnish an account of the action of the principal pharmacopœial drugs, even supposing no new element to be introduced. To return, in the hands of Dr. Harley this study of the physiological action of drugs has proved eminently successful, by it he has shown us that we have medicines which definitely act on certain parts of the nervous system, leaving others untouched, clearing away a number of useless preparations because absolutely inert, although when tested on diseased states they had been over and over vaunted as yielding wonderful results, it is hardly credible that we have been using for severe painful affections, for years, and are still using an extract of conium, when it might so easily have been proved to be utterly worthless. Again, take the results obtained by the committee presided over by Hughes Bennett, on the cholagogue action of mercury, podophylline, and taraxacum, if it be true that 99 out of every hundred allopathic practitioners still hold that the biliary secretion is increased by these drugs, on what ground can they support their assertion, or how long is the question to remain undecided, if it be not decided by its action on animals? These drugs are not necessarily to be disregarded in diseases of the liver, because they do not act on the biliary secretion, but at least any value they have, ought to rest on experience, apart from any support afforded by their supposed freeing of the *secretions*. No doubt it will be said that diseased livers might be stimulated to secrete by an agency which is without effect on a healthy one, a proposition I am inclined to believe myself, and for reasons I will state farther on, and that experiments on animals are not conclusive as to the action of a drug on man. An illustration of the truth of this I will take from an interesting paper of Dr. Weir Mitchell's, in which he shows that pigeons can be given almost any amount of

opium. He says, "in a pigeon which within the last two days had swallowed 42 drops of black drop I gave between 2 and 6 p.m., 21 grains of powdered opium in soft pills of 3 grains each. Except the usual tendency to remain quiet none of the common evidences of opium poisoning appeared, and the pigeon was well and active next day." This result is due to the non-development of cerebrum in the animal upon which the experiments were made, and could not be the case in the liver of a dog which is well developed, though no doubt many circumstances might cause a similar result.

Lastly, among others, we may refer to the effects of bromide of potassium as an illustration of what can be done by the physiological method to discover the real action of remedies.

1st. It is found that it acts as such and not from the effects of either of its constituents.

2ndly. That it produces extinction of the reflex power before its action on the other parts of the nervous system are obtained.

3rdly. That it contracts the small blood vessels first, succeeded slowly by a gradual dilatation of them.

4thly. That it gradually diminishes and finally destroys voluntary motor power.

5thly. That it is an anæsthetic acting as such, not so constantly as on the motor function.

6thly. That in many cases respiration is slowed and even stopped, the quantity of CO_2 formed of lessened, and temperature slightly lowered, and that it has *no action on secretions.*

In this way, then, we obtain a set of bodies which, having peculiar affinities for certain tissues, act on them in the order of those affinities, to perform results which the sceptical would have at first laughed at the possibility of. I merely mention again a few things, some pointed out on the last night of meeting by Dr. Smith, as illustrations of what can be done by this method.

In bromide potassium, we have a drug acting on the reflex function of the cord; in conium, on the motor power of the brain; in curare, on the extremities of the motor nerves; in chloroform, primarily on the ends of the cerebro-spinal sensitive nerves; in belladonna, a stimulant to sympathetic system, contracting the blood-vessels; and in strychnia and the action of cold on the sympathetic ganglia, a method of dilating them.

Now, though we cannot apply these to stop pathological processes on account of our knowledge being deficient both in those processes and in the action of the drugs, yet the time, I believe, will come

when at least in some of the earlier steps we will be able to *arrest disease* by the direct action of drugs whose effect we can proximately foretell before using them.

There is one objection to the use of this physiological method which often, I believe, occurs and requires to be guarded against. I have alluded to it before, namely, that a body acting one way in health, may, under certain limitations, act differently in disease.

I will take an example—Prof. Parkes has been engaged in investigations on the effect of alcohol on the functions of the healthy body more elaborate than any previous ones, in which he has upset a number of conclusions arrived at as to the action of this agent on the excretions and chemical changes in the body. He proves that alcohol does not affect the temperature, that it does not lessen it, does not increase weight nor affect the elimination of nitrogen from the bowels or urine. Simultaneously Prof. Binz published a series of observations in which the amount of temperature in fever has been sensibly and continuously lessened by the use of alcohol, and this is in concurrence with the experience of most physicians. I have frequently, in the fever wards at the City of Dublin Hospital, traced distinct and constant falls of temp., ·5 and ·7 of a degree, after ½oz. doses of whiskey. Thus these two sets of experiments have arrived at apparently opposite results, one in the healthy and the other in the diseased condition.

Another example may be taken—digitalis is found in its action on healthy individuals to diminish the amount of urea in the urine, and yet it is used in so-called uræmic poisoning to eliminate it from the system, and practically it does so. Every one is familiar with the enormous effects which sometimes result from the use of poultices of digitalis in these cases, and I am sure we will never be called on by a higher knowledge to give up practising with either the alcohol or the digitalis. I remember a little boy, a short time ago, whose kidney a *post-mortem* examination proved to be in a state of parenchymatous nephritis, in whom, when almost comatose from suppressed urine, a poultice of and the internal use of digitalis produced a copious secretion almost solid with lithates followed by temporary remission of all the symptoms; nor is the explanation far to seek, the effects of the medicine being the unloading of the tubes, and consequently the free passage of urine, although on the direct production of urea the medicine has a lessening influence.

But these differences can always be explained, and though they

detract from the ease in the application of the physiological method, they in no way vitiate the rationality of its applications

If these facts produce on your minds the result they have on mine, the condition of our system of therapeutics will be this:—

We do not know except for a few diseases their natural course; in consequence, we cannot give at present a fair answer clinically to the question—what value are our drugs?

We do not know accurately the effects of our drugs on the healthy, and therefore cannot apply them with success to the diseased. We do not know except for a few remedies the effects upon their action of different conditions of the system.

We cannot at present combat directly, pathological processes on account of the insufficiency of our knowledge both of the disease and the medicine.

We must, therefore, confine our clinical medication to the relief of distressing symptoms and efforts to obviate the tendency to death in a particular *form*—ex. by asthemia, &c.

The method by statistics up to the present has been applied in so loose a manner as only to lead us astray. In order to make it useful we must exclude cases of slight severity or of structural degeneration far gone and inactive.

Medicines have been so combined in their uses, and the doses have been so *variable*, small and without known physiological effect, that it has been impossible to ascertain which medicine has had the effect, or how did it, if any.

The action of medicines even on diseased states was undertaken at a time when diagnosis was in many cases so imperfect as to vitiate the results completely, and render it necessary to re-investigate their effects.

And now, before concluding, let us remember that though we are in a period when many old landmarks have been removed, when we wander through untried paths in search of fresh and more reliable remedies for disease, we are in the heart of much earnest, encouraging work. Any one who will study the history of man will find that the periods of the greatest scepticism have mostly been those of the truest advance; doubt and dissatisfaction go hand in hand with efforts to escape from their trammels, and we can look with no little complacency on the state of our practice in comparison to the time when the entire medical opinion of the civilized world was one without a dissentient voice, one in

a sleep of folly from which our forefathers have done much to emancipate us, and which it will be our own fault if we settle quietly down again in.

There is much work before us in this field, which requires, indeed, energy, care, and a great desire and love for truth, as such in the workers but which is tangible, and is not freighted with the lives of those we make our experiments upon.

ART. XXIV.—*Some Remarks on Structure and Function, in Advocacy of the Spiritual Origin and Direction of Life, in Contradistinction to Blind Brute Force and Automatic Will.* By HENRY MACCORMAC, M.D. Part I. (As read before the Ulster Medical Society, 17th December.)

ANATOMY, human and comparative, general and special, in its present state of approximate perfection, is the result of the labours of a long series of inquirers. Aristotle, that great genius, made it, among so many other matters, the subject of his careful attention. He did not understand the action of the muscles, although he noticed their development, as it varied in different classes of animals. On Erasistratos of Ceos devolved the credit of determining that, in order to act, a muscle must needs contract;[a] also the discredit if, indeed, it be true, of dissecting men alive. Galen pointed out that blood filled the vessels and the heart, showed how lesions of the nerves and medulla implicate muscular action, and even induce paralysis. Passing over some fourteen centuries of barbarism and contention, we come to Berengarius, of Bologna, a city which he was constrained to leave, charged with having examined the bodies of a couple of Spaniards ere the vital spark had fled. This able anatomist, by his multiplied inquiries, paved the way for his successor Vesalius who, singular to relate, incurred a like evil repute in respect of a Spanish nobleman by him erroneously imagined to be dead, and only escaped through the intercession of Philip II., on condition of a journey to Jerusalem, perishing on his way back. Servetus first appears to have made known the cardio-pulmonic circulation, and likewise the depuratory action of the lungs. Eminent as a philosopher, happening to differ on a speculative point from a no less eminent divine, he was by him and others, infamous and almost incredible

[a] Marey, Du Mouvement dans les Fonctions de la vie, p. 9-11.

to relate, entrapped and burnt alive, a frightful example of the irony of fortune, in that he who lavished the divine gifts of genius on his species should experience destruction as the requital in return. Malpighi distinguished himself in numerous particulars, among the rest by the employment of the microscope in anatomy. Harvey's demonstration of the greater circulation, at or about the year 1618 or 1619, was simply and truly the result of the admirable application of anatomical knowledge. He was not, indeed, the first to detect the valvular structure of the veins, but he was clearly the first to deduce from that structure the immensely important inference that registers his name in the records of our kind. Valsalva, and his pupil Morgagni, both made amplest contributions to anatomical science in their time. The discovery of the pancreatic duct has been ascribed to Wirsung, as well as to Eustachius, who made known to us in detail the tubes which bear his name. Pecquet detected the chyliferous ducts, while Rudbeck and Bartholinus determined the constitution of the lymphatics. Ruysch was indebted to Swammerdam for a knowledge of the material which he employed in making his admirable injections. Having disposed of his first museum to the Russians, Ruysch, who had been meanwhile disabled by a fall, employed himself in the formation of a new one, a task in which, it may prove of some interest in these days of lady doctors to remember, he was most ably assisted by his youngest daughter. Not those great men only whom I have named, but many others, as Albinus, Winslow, Vicq d'Azyr, Stahl, Hoffmann, and others, by their zealous toil and persevering industry, very greatly added to the sum of our attainments. Of Haller, indeed, it is difficult to speak in terms of moderate eulogy, so great were his achievements in almost every branch of anatomy and physiology. Few observers, however, whether English or foreign, surpassed the Hunter brothers in respect of the interest and importance of their inquiries. Their collections, the Hunterian Museum of London in especial, are replete with evidence of their vast research, and can never, I think, entirely lose their value, however far reaching may prove the results of future inquiry. The world has endorsed the well earned repute of these our medical fathers, some of whom, though now securely illustrious, paid dearly enough for the position to which it has been their great good fortune to attain.

Anatomy is the science of organization, the knowledge of what

Haller tersely styles the fluid and solid parts.[a] Organic unity is the great, the momentous fact of living nature. Initiated by the Greeks, its truth seems now admitted by every investigator. There is no origin or beginning in the body, observes wise Hippocrates, the parts, all of them, are equally, both beginning and end, for in a circle there is no beginning.[b] A like remark might be applied to the living functions which, in fact, are solidary of each other, without beginning as without end. Geoffroy Saint Hilaire attached less moment, however, to the organs, with their variable forms and functions, than he did to their composition, together with, as he named them, the four laws of organic life, to wit, the theory of analogues, the principle of connexion, elective affinities, and the balance of the organs themselves.[c] Every organism, indeed, undergoes, or if you will, performs, a series of acts from birth to death, various, however, as the conditions of life and organization themselves vary.[d] According to Milne Edwards, speaking of the constituents of the living frame, the form and disposition of the different tissues and dimensions of the organic molecules or globules, are alike in all animals. The diameter of the molecules he lays down as $\frac{1}{300}$ of a millimetre.[e] The fundamental material of all the tissues, nerves, bones, muscles, and connective tissue alike, Dr. Lionel Beale, who has isolated it by submitting it to the action of the ammoniacal solution of carmine, terms bioplasm, or living plasm, germinal or growing matter.[f] It is particularly abundant, he tells us, in the muscular tissue of the heart, where new tissue is incessantly formed to replace that which so rapidly wears away.

Bichat reckoned up fourteen several tissues, leaving out, singular to relate, the cellular tissue, most important and generally diffused of any, since it is, in fact, the common matrix or mould of that by which and in which each and every structure is imbedded, pervaded, and enclosed, the basis of bone, the bulk of the viscera, arteries, veins, nerves, and absorbents.[g] The occurrence of emphysema

[a] Primæ Lineæ, cap. i.

[b] *πάντα ὁμοίως ἀρχὴ και πάντα τελευτή, κύκλου γαρ γραφέντος ἀρχὴ ουκ εὑρέθη.* De Locis. The subject is adverted to by Le Clerc, Part i., Book ii., of his history, as well as by his continuator Friend, and writers on the history of medicine generally.

[c] Mémoire sur la Structure des Tissus, cited by Meckel.

[d] Marey, Du Mouvement dans les Fonctions de la Vie, p. 8.

[e] Physiologie.

[f] The Mystery of Life, p. 36.

[g] Drummond, First Steps to Anatomy, p. 44.

after certain wounds, the custom once common of fraudulent butchers in blowing up meat, and of horse chaunters in filling up the hollow above the eye in aged horses before selling them to unwary customers, as well as the practice of a wretch in London who used actually to blow up the subcutaneous cellular tissue in a little boy, whom thus distended, he exhibited as a show, all go to prove the great pervasiveness of the cellular tissues. Van der Hoeven reduces all the tissues to eight, as the conjunctive, vascular, adipose, nervous, corneous, cartilaginous, muscular, and elastic.[a] The cellular, vascular, and nervous, Meckel esteems the fundamental tissues.[b] Rudolphi unites with Haller in dividing animal structures simply into solids and fluids,[c] assigning, however, as Aristotle did before him, the preponderance to the fluid.[d] If one, however, would have other than the merest general ideas, one must, perforce, isolate the tissues.[e] No animal, save man, the anaplotherium excepted, observes Cuvier, has an uninterrupted series of teeth.[f] And man, too, I would say, is also the only one in which the incisors form a right angle with the plane of each jaw. The facial angle on which Camper laid so much stress, and which the Greeks so greatly exaggerated in their ideal statuary, according to Meckel, is seventy-five in the Mongolian and eighty degrees in the Caucasian man. Our erect stature, although at a great expenditure of means, may be said to be exclusively our own. And half the head, which is the case with no other creature, a fact, so far as I know, first stated by Winkelmann, in his admirable essays on art, is placed above the plane of the orbit. In other respects man, as Huxley insists,[g] shares certain morphological conditions with other animated organisms, but, when classed, each group displays structural characters common to the members of that group only, and isolating it from all others. Thus, among the vertebrates, mammals possess two occipital condyles, a well ossified basi-occipital, each ramus of the mandible composed of one bone articulated with the squamous element of the skull, and non-

[a] Handbuch der Zoologie, Band i., s. 9.

[b] Anatomie Générale, Jourdain and Breschets tr., p. 103, 121.

[c] Grundriss der Physiologie, i. Band, s. 66.

[d] *πολὺς γὰρ ὑγρος*, De Generatione Animalium.

[e] Vous pouvez considerer chaque organe en masse, mais il est absolument necessaire d'en isoler les tissus si vous avez envie d'analyser avec rigeur sa structure intime. Additions á l'Anatomie Generale de Xav. Bichat, Elemens Anatomiques.

[f] Regne Animale.

[g] Introduction to the Classification of Animals, p. 8.

nucleated red blood corpuscles. The basis of medical science, observes Pinel,[a] reposes on facts. It is so also in anatomy, it is so in everything. But, as Aristotle says, we must proceed from the simple to the complex, from the better to the less well known, just as in dealing, for so he puts it, with[b] coins, ὥσπερ νόμισματα. Science, in truth, is progressive ever. But while the inquirer busies himself with one portion, the rest is advancing ceaselessly, so as to render it difficult to retain everything within his grasp.

Without impeachment of the claims of any, a more remarkable person than Bichat has never, perhaps, adorned the great cause of anatomical research. And however wellfounded may prove the undoubted merits of our Homes, Hewsons, Pritchards, Carpenters, Goodsirs, Busks, Bowmans, Hassalls, Ralphs, Bennetts, and others, to Bichat is certainly mainly owing the foundation of modern general anatomy. Born in the department of the Ain, he removed at an early period of his career to Paris, where, having been one day called upon without any special preparation or previous notice, to report, *viva voce*, a lecture of Desault, the person charged with that duty having failed to appear, he acquitted himself so admirably that the great surgeon at once received him in his house and treated him as a son. Struck with the masterly manner in which Pinel had treated of the serous and mucous membranes, Bichat himself took up the topic, and, some time after, published his book on the *Membranes*, followed, in succession, by his *Life and Death;* and lastly, his *General Anatomy*.[c] A fall down the stairs of the Hôtel Dieu, resulting in fever and death, to the infinite regret of every one, brought Bichat's indefatigable energies to a close, and, at the comparatively early age of thirty-one, robbed the world of the ablest and most philosophical anatomist for his years that had ever lived. His writings, though somewhat over copious, are replete with keen inference and admirable research. It was Bichat's ambition to found medicine on anatomy, and doubtless, had he lived, he would at least have striven to compass his design. Those who have nothing to say, observes one of his biographers,[d] are prone to tell you that everything has been said. It is quite otherwise, however. Before the man of true genius and observation the fields of discovery extend far and wide.

[a] Nosographic Philosophique, 6iéme ed., Tome i,, p. xciii.
[b] Historia Animalium, lib. i., cited by Van der Hoeven.
[c] Anatomie Generale. Nouvèlle edition.
[d] Notice Historique, par Scipio Pinel.

To him, indeed, nature delights to unseal her hidden treasures, treasures that remain hidden to the idle and inapprehensive. As it is, Bichat has left amplest materials for thoughtful investigation, and, as thus, has bequeathed a heritage which, or at least much of which, must, I think, for ever endure.

It seems to be a condition of progressive thought and action that the inquirers of one age shall conduct a series of investigations to a given point, when others shall take them up, and, perchance, achieve results which to the first are simply unthought of and unknown. Such, in effect, have proved the issue of the labours of Dr. Lionel Beale and M. Charles Robin, both ardent cultivators of histological and general anatomy, as contrasted with and supplementing those of Xavier Bichat. Older anatomists were at least excellently well acquainted with the general outline, the bones, muscles, nerves, vessels, and viscera of the living frame. Bichat, however, showed that there were other things to be considered also, and came forward with his famous, but at the same time somewhat artificial, division, into symmetrical organs or those of animal life, and the non-symmetrical or those of organic life, the first comprising the brain, spinal marrow, their nerves, and appendages, the second including the vascular system, great sympathetic, digestive, respiratory, and renal apparatus.[a]

Dr. Beale's most recent exposition appears in his work, *The Mystery of Life*, setting forth his reasons for dissenting from those who esteem structure and function merely correlations of matter and the ordinary material forces of nature, resultants, in fact, of the non-living world, instead of, as he states in his treatise on protoplasm, a power, force, or property of a special peculiar kind, temporarily influencing matter, and its forces, but different from, and not merely correlative with, them. I most cordially concur with Dr. Beale that Life is not the result of the brute forces of nature, and proceed at once to cite from his truly important remarks on bioplasm. Connected with every tissue, he tells us,[b] are certain oval, circular, or irregular masses. These soft structureless masses, differing from matter in every other state, he terms bioplasm. By subjecting tissues, immediately after death, to the action of carmine in ammonia, bioplasm is obtained of a deep red colour, while the tissue, nerve and muscle alike, remains colourless. The aspects of bioplasm, thus coloured, Dr. Beale has

[a] Meckel, Anatomie Generale, Jourdains tr., Tome i., p. 94.

[b] Mystery of Life, p. 35.

set forth in his admirable plates. This important constituent of living bodies, being normally colourless, has even been passed over as of no moment, its presence regarded as accidental, and every trace of it omitted in text-books. Five times as many bioplasts exist in the heart, owing to the activity of the changes in its muscular tissue, as in ordinary muscular fibre. Some bioplasts go to form muscle, others nerve. Others again form connective tissue, but nerve bioplasm cannot be distinguished from that which makes up muscle, or either of them from connective tissue bioplasm. At an early period indeed, when the heart, in this instance speaking of the hyla or tree toad, is represented by spherical bioplasts, there is not a vestige of muscular or nervous tissue, but merely the living matter by which alone the formation of such tissue is possible. As development advances, however, the bioplasts recede from each other, and tissue appears. But the bioplasm does not disappear. It continues to take up nourishment, which is converted into bioplasm, and thence, *pari passu*, into tissue. Into Dr. Beale's further remarks on the distinction which he draws between life and mere physical force, I need not here further enter. Not only on these grounds, however, but in respect of his remarks on minute nerve structure, his treatise deserves the most careful study.

The humoral pathology and physiology once so extensively in vogue, was perhaps too summarily dismissed. In strictness, the humours, the blood, are solidary, in fact, part and parcel, in respect of life and function, of the tissues. The solids are made up of various ingredients, principles, in truth, anything but homogeneous, which M. Robin divides into three classes.[a] They constitute the simple principles of biology, not less essential in the interpretation of living facts to the anatomist, than are those of chemistry and natural philosophy to the student of physics.[b] The determination of muscular action and creation of power through the mere fiat of the will, is one of the most wondrous particulars, nay, the most wondrous particular, of animal life. It is a power shared by the whole animal creation, but how it takes place we neither can know nor imagine. It is accomplished, at least speaking of the voluntary muscles, through no brain action, no conscious or unconscious cerebration, seeing that there is nothing of the sort considered as a mental act, but through a

[a] M. Papillon, Travaux de M. Robin, Revue des Deux Mondes, July, 1870.

[b] Robin et Verdeil, Traité de Chemie Anatomique et Physiologique.

direct volition of the intelligence. Felted and welded together, the anatomical elements make up the basis of the individual. They comprise the variously formed cells, fibres, and tubes. Cell structure, excessively diffused, includes the red and white blood corpuscles, the elements of bones and nerves, with those of the central nerve substance, ganglions and epidermis. Elementary muscular fibres vary as they belong to those of organic or animal life, to borrow the language of the writer of the very able summary in the *Revue*. The connective and elastic tissues are also constituted of special fibres. There are tubiform elements in the nerves of animal life, white filaments of the great lymphatic, the myolemma which envelops the fascicles of the muscular fibres of animal life, capillaries, glands, parenchymatous structures, and lastly, the tubes which go to make up the nerves.

M. Robin discriminates carefully between the tissues and their anatomical elements, invisible to the naked eye, wherein life's fire doth mainly burn. The red blood globules are of an infinite and hitherto quite unsuspected complexity, form indeed the immediate element, as Claude Bernard terms it,[a] of our ever-renewed organic perfectibility. The histological elements which compose the living organism, as formed by the red globules, are not laid down at random.[b] Like elements combine with like to form tissues which, in their turn go to make up organs of severally varying structure. complexity, and use.[c] Assimilation and disassimilation, homoiomorphy and heteromorphy, absorption and secretion, substitution and interpolation, proceed on a predetermined plan ceaselessly and without pause together. Old materials are shunted off into the venous current, while, again, new blood discs for instant use are formed within the arterial current itself.[d] Philosophical anatomy strives as best it may to explain the processes of growth and decay, amid two orders of forces, vital and general, at work ceaselessly, and, whatever may be said, in only too many respects one as little known and understood as the other.[e] The same

[a] Element de Perfectionnement Organique. Leçons sur la Physiologie et la Pathologie du Systeme Nerveux. Tome i., p. 49.

[b] Les elements histologiques ne sont pas entassés pêlemêle et sans ordre. Les éléments de même nature s'assemblent pour former les tissus, comme les tissus de nature differente s'associent pour constituer les organs. Id., p. 63.

[c] Beale, Structure ard Growth of Tissues. Lecture I.

[d] Hassall, Microscopic Anatomy. Vol. i., p. 80.

[e] La'natomie philosophique suit un organe à travers toutes les variations qu'il a pu éprouver. Ducrotay de Blainville, Principes d'Anatomie Comparée, p. 5.

thought is somewhat differently expressed by Chevreul, when he observes that living bodies are so much the more complex by reason of adding general physical qualities to the special conditions of their vitality.[a] And thus it is that the sum of the latter may be divided into consenting and opposing forces of complex and varying energies.[b] For the principle of vitality, life itself simply designates organic activity considered as a whole.[c] There is an uninterrupted correlation not only of the interior parts but of the inward and outer organs, in such wise that one proves, as it were, the virtual exponent and unvarying outcome of the other.[d] The able zoologist or anatomist, some Cuvier of the present, from a given organ or merest fragment, were it, will detect genus and species, even age and sex, as well as the distribution in the order of space and time, with almost unvarying certainty and correctness. The general tendency of histological inquiries, what Leydig would term the comparative doctrine of tissues,[e] observes Müller,[f] has been to refer to cell formation the actual genesis of the tissues themselves. This process, however, must surely ensue in the very centres of nutrition, those minute cellular structures, as Goodsir terms them,[g] already existent in the texture of organs. The sections of minute structure which Leydig gives, as in regard of the dog, calf, eel, bird, and *cœcilia annulata*, in particular, are, I think, of great interest, and go far, perhaps, to constitute his important treatise, as was that of Swammerdam *longo intervallo* before him, a veritable outline of the vast superficies of living nature.

Evolution and nutrition belong to each and every anatomical element, and yet, one is very distinct, indeed, from the other. For anatomical elements, when they first appear, are not what they afterwards become. They gain in volume, assume forms more perfect, which again in turn, pass away. In fine, organic phenomena, in their relations to the anatomical element, may be summed up under four divisions, to wit, nutrition, evolution,

[a] Journal des Savans, 1847, cited by M. Robin.

[b] Jede Spannkraft kann man sich in eine spannende und eine hemmende Kraft zerlegt senken. Wundt, Lehrbuch der Physiologie, p. 47.

[c] Le nom de vie ou de vitalité est reservé pour designer le mode d'activité propre à 'organisme consideré dans son ensemble comme un tout unique. Robin, Histoire Naturelle de Vegetaux Parasites, p. 56.

[d] Correlation telle que la disposition anatomique des parties internes se traduit au dehors par le disposition des parties externes. Id. propre, p. vii.

[e] Lehrbuch der Histologie, Vergleichende Geweblehre Vorrede.

[f] Physiology, Balys tr., p. 1,428.

[g] Pathological Observations, Centres of Nutrition.

contractility, and innervation. In some mere human machine one motor commonly suffices, but in the living organization many factors, numerous forces, endless complications, countless elements, are at work constantly in the effective, ceaseless furtherance of the different processes of life.[a] Every organized substance, according to M. Robin, determines in its neighbourhood new anatomical elements, forms of activity, by means of segmentation, gemmation, and the fruitful action of the blastema. A minute nucleus, surrounding itself with solid matter, continually elaborates those cells, fibres, and tubes which contribute to, in fact, constitute, the never ceasing manifestations of organic life and being. Blastemas are various as there are tissues, in other words, the anatomical elements of each given tissue permit amid their interstices those generative fluids, from which similar elements spring. Organic corpuscles, which but an instant before had no existence, are produced in ceaseless molecular affluence, how, we know not, unless, as I conceive we are bound to do, we ascribe it to a predetermined will and power which, in the entire living structure as in its several parts, act in virtue of an archetype, devised by a Divine intelligence and unalterable skill.[b] The amœba, for example, takes food into its interior, and, without a single visible digestive organ, in virtue of some inexplicable power, selects what it needs, while it rejects what it can do without.[c] The possession of life, indeed, were it in the very humblest organism, some mere rotifer or gregarinid, imparts an interest which as thus it shares in common with the greatest. Van der Hoeven, in his elaborate treatise, the result, he tells us, of the labour of six of the best years of his existence, expresses himself with emphasis on the subject of the almost inconceivable perfection everywhere manifested in the structure of the minutest creature of the organic world.[d] Those who under a high magnifying power have watched

[a] Les parties intérieures sont les appareils qui se subdivisent en organes lesquels se groupent en systèmes divisibles en tissus et humeurs susceptibles d'être ramenés à un certain nombre d'élémens anatomiques ou organiques et de principes immédiats. C'est à ce tout formé par la réunion de toutes ces parties, qu'on donne le nom d'organisme.—Robin, Hist. Nat. p. 27.

[b] La connaissance d'un être tel que l'homme, a existé avant que l'homme fit son apparition, car l'intelligence divine, en formant l'archétype, avait la prescience de toutes ses modifications.—Owen, Principles of Comparative Osteology. French Ed. p. 12.

[c] Nicholson, Manual of Zoology, Nature of Life.

[d] Die Natur hat in dem Baue des thierischen Körpers eine unbegreifliche Kunstfertigkeit an den Tag gelegt. Nicht nur der Körper in gauzen, ja nicht nur seine

the varied movements of living matter will not, says Beale, be easily brought to believe that such phenomena are due only to physical and chemical agencies.[a] In any and every case, however, having done our best, we must, perforce, fall back on the implicit declarations of empirical experiential law, that is to say, on unity of plan with diversity of detail. Happily, says Mr. Huxley,[b] there is a criterion of morphological truth, a sure test of all homologies. the study of a real and not merely an imaginary development, proves that the doctrine of unity and, as I would add, of a predetermined plan, is not mere fancy, but the expression of absolute law, the utterance of well established facts.

M. Robins' doctrine as to the genesis of anatomical elements in the heart of the blastema itself, is not admitted by every one, for example, not by Virchow, who would refer the origin, or at least the extension of cells, to proliferation, and of other elements to metamorphosis. But anatomical elements, not to speak of morbid products, are often so dissimilar, muscular fibres and nerve tubes resemble each other so little that one hesitates to concede their production to cell origin. There is, in truth, genesis by substitution, that is to say, the wasted elements are redissolved in the circulating medium and dissipated, while others are taken into their place. In fine, the anatomical element is the true arena, the very keystone, so to speak, of material existence, enwraps the secret springs, those hidden energies, whose multiple sum we term life. M. Robin lays claim to the discovery of lymphatics in the brain, the structure of ganglions, the distinction between nerve tubes and cells. A remarkable feature of his views is that morbid growths, some, if not all, tumours, cancers, cysts, and others, are not so much new growths as a sort of hypergenesis of existant normal tissues. He is very surely in error, however, as to tubercle, since this is not the extension of any living tissue, whatever, but simply, as I assume to prove and show, the deposit of unoxidized retrograde carbon, unoxidized and unexcreted by reason of the habitual respiration of air more or less prerespired. In sum, M. Robin has rehabilitated the humoral pathology, not, indeed, the blind irrelevant pathology of the past, but a pathology

gröbern Theile, Sondern selbst die Kleinsten Theile der Organe sind Werkzeuge und das anatomische Messer zeigt uns bis zur einfachsten Fäser nichts als zweckmassig-eingerichtete Theile.—Handbuch der Zoologie. Band I., S. 9.

[a] The Mystery of Life, p. 71.

[b] On the Study of Zoology.

purified and adjusted to living realities. The blood, in truth, is the arena of existence, the laboratory wherein transformations ensue, which no chemist hitherto has weighed in his balance, no physiologist detected with his lens. There is no virus either, as apart, the fluids only, M. Robin affirms, become virulent. In other respects, the fast and fluid parts are solidary of each other, since what is fixed at one instant becomes fluid in the next, and conversely.

There is no vital principle, then, as apart from the play of phenomena under providentially regulated control. But a vital principle, as thus, there most surely is. Matter, force, the sun's light and heat, the electric spark, *per se*, no more originate and discharge the processes of life than they do the confection of our food, the putting on or off of our daily attire. In every thing we witness the, in fact, miraculous harmony and consent of the various processes of the organism. In these controlled, indeed, and regulated by a higher power, may we alone discover the only true solution of the enigma of life. Everything, this premised, may be said to result from the play of the energies of the ultimate particles of the organism,[a] otherwise, as to the principle of the principle, the causation of the cause, the inner mysteries of life and being, we know or can know directly nothing. They are there, but reach them we may not. We can do little other or better than observe, but yet with all our insight, our utmost care. In us and about us there exist nothing but wonders. We proceed a little length, we attain to some faint inklings, but the arcana of existence we are all unable to resolve. And yet to us none the less belong the city of man's existence, the habitat of a living soul. Ours, in truth, is a right marvellous organism, but, much as we may talk and write about it, of the intimate essence and secret springs of that organism we shall never here, I fear, know more than we do of the state of the dwellers in Arcturus or the condition of the inhabitants of Algarob.

[a] De l'Approximation des Parties Organiques et de l'Organisme à l'Accomplissement d'Actions determinées, cited in the Revue.

ART. XXV.—*On Decapitation as a Mode of Delivery in Cases of Shoulder Presentation, in which Version cannot be safely Effected.* By GEORGE H. KIDD, M.D., F.R.C.S.I.; President of the Dublin Obstetrical Society; Honorary Fellow of the London Obstetrical Society; Corresponding Member of the Gynecological Society of Boston; Obstetric Surgeon to the Coombe Lying-in Hospital. Read at the Dublin Obstetrical Society, 11th February, 1871.

CASES of arm and shoulder presentation in which version cannot be performed without exposing the mother to extreme danger, though of rare occurrence, are sufficiently common to make it necessary that we should be prepared to deal with them when they arise.

These cases are three-fold:—1st. "In some cases," to use the words of Denman, "the shoulder is so far advanced into the pelvis, and the action of the uterus is, at the same time, so strong, that it is impossible to raise or move the child, which is so forcibly impelled by the pains as to overcome all the power we are able to exert. This impossibility of turning the child," Denman proceeds to say, "had, to the apprehension of writers and practitioners, left the woman without any hope of relief;" but he goes on to show the uterine efforts will sometimes overcome the difficulty by the production of a spontaneous evolution.

In a second class of cases the difficulty arises from the firm contraction of the uterus. The liquor amnii is evacuated before dilatation of the os has taken place, and the uterus becomes moulded to the child's body. The undilated state of the os prevents the impaction of the shoulder, which forms such an obstacle to turning in the first case. It is, in these cases, quite possible to introduce the hand, seize a foot and bring it down into the vagina, but the child does not turn, the head remains fixed in the iliac fossa, and the uterus may be torn in the efforts to complete the delivery.

3rdly. Cases of transverse presentation with extreme narrowing of the pelvis.

In the first and second of these cases the child is almost always dead, the compression of its own body and of the placenta having put a stop to all circulation, so if the heart's sounds cannot be heard we should not persevere in efforts to turn, nor yet wait for the possible occurrence of spontaneous version or expulsion, but at once lessen the child by the most effectual means in our power.

The operations recommended in these cases are two, evisceration and decapitation. Of these, evisceration is the more modern, and, sometimes, the more easily performed; but it is generally, in the words of Pajot, "an operation most dangerous and most difficult."[a] Decapitation is a much older operation. It was described by Celsus, and many writers since his day, yet never came much into use, because, probably, of the imperfection of the instruments for carrying it out; but most of our best modern authors recommend its adoption in the warmest terms. Sir James Simpson says of it, "The expediency of ever having recourse to decapitation has been questioned in some quarters; but in cases of transverse presentation where the waters had long escaped, and the uterus had become entirely contracted around a dead fetus, he believed that delivery could be accomplished with more safety to the patient, and more ease to the practitioner, by decapitating and diminishing the size of the fetus, than by attempting the, in such cases, always difficult and sometimes hazardous operation of podalic version."[b]

Tarnier says "Decapitation only is capable of fulfilling all the indications when in a case of presentation of the body it is impossible to turn."[c] Scanzoni, Davis, Ramsbothom, and Barnes, also advocate the operation in the strongest terms; the last named speaking of it as one of "extreme importance capable of bringing almost instant relief and safety to the mother." Such testimony in its favour demands that we should give the operation our most careful consideration.

The instruments that have been used for dividing the neck of the fetus, in utero, may be arranged in three classes:—1st. We have hooks with variously devized cutting blades. The first of these, and the model on which the others of this class have been formed, is the decapitating hook of Ramsbothom, senior, which is, in fact, the ordinary blunt hook, with a cutting blade fixed in the concavity of the curve, and which resembles much the instrument described by Celsus. Davis, Scanzoni, Jacquemier, and others have modified this hook by the introduction of a guard or sheath, or of cutting blades, made to move by screws and levers in the handle, for dividing the neck. Dubois uses a pair of strong scissors with which he cuts through the neck, bit by bit, from below upwards. Professor Heyerdahl, of Bergin, adopted a different plan. In 1855, he

[a] Pajot. Archives Générales de Médicine, 1865, Vol. ii., p. 257.
[b] Edinburgh Medical Journal, September, 1862, p. 284.
[c] Notes to Seventh Edition of Cazean's Midwifery, American Edition, p. 1060.

invented an instrument consisting of a handle and hollow stem, together, 14 inches long. The extremity of the stem is curved and forms about a third of a circle. The instrument is hollow, and a strong wire passes up the centre. The wire is pushed up by means of a button sliding in the handle, and at its extremity there is a little knob which closes the open end of the stem. The instrument is used thus:—After it has been passed round the neck the internal wire is pushed upwards, and this projects the knob round and beyond the other side of the neck. A loop of string can now be passed round the knob, and, on withdrawing the instrument, a cord is thus left round the neck, with which a chain or wire rope is drawn round, with which the neck is sawn through, and, to avoid mutilation of the soft parts of the mother, the rope or chain is crossed in the operation.[a]

Without appearing to have known anything of Heyerdahl's method, Pajot and Tarnier have adopted modifications of it. In the Archives Générales for September, 1865, Pajot published a paper on presentation of the shoulder, where there is extreme narrowing of the pelvis, and on a new method of embryotomy. This new method is virtually Heyerdahl's, and consists in sawing through the neck with a cord. To get this cord round the neck and, at the same time, to avoid the multiplication of instruments, he makes use of the handle of his forceps, which is very much bent, and the extremity of which he has perforated, and through this perforation he passes a cord, on the end of which is a leaden bullet. The cord is pulled till the bullet is closely applied to the end of the bent handle of the forceps, which is then passed up into the uterus and hooked on the child's neck. The cord is now relaxed so as to allow the bullet to fall and roll down into the hand of the operator; but if the compression of the parts should prevent the ball from finding a passage, a simple pressure on the fetus with a finger or the bent hook will immediately determine the formation of a sort of gutter in which the ball will at once engage itself. As soon as the cord is fixed in its place the instrument is withdrawn, and the ends of the cord are passed through a speculum, which is introduced into the vagina to save it from injury, and by drawing the cord rapidly backwards and forwards the neck is cut through in a few seconds. If the neck should be inaccessible, the cord may be placed round the body, between the crest of the ilium and shoulder blade, but it

[a] Catalogue of Instruments exhibited at the Conversatione of the Obstetrical Society of London.

will occupy four or five minutes to effect the division here. Pajot's colleague, Tarnier, seeing Pajot's experiments, suggested the use of an instrument, such as is used for plugging the posterior nares (Balloc's Sound), for carrying the cord round the part of the child to be divided, which was the one feature wanting to make it virtually, and to all intents and purposes, Heyerdahl's operation.

During a visit to Paris, in November, 1869, I procured Pajot's instrument, and determined to give it a trial on the first suitable occasion, but it was not till the 8th January last that I had an opportunity of doing so. On that day I was asked by Dr. Harley, of Baggot-street, to see a poor woman, of whom he gave the following history. She had had six children, and always with difficult labours, owing to a narrowing of the brim of the pelvis. When he first saw her she had been three days in labour, and the hand and arm had been lying outside the vulva for 24 hours. He had passed his hand into the uterus, laid hold of a foot and brought it down into the vagina, but by no effort, that he thought justifiable, could he get the child to turn. When I saw her I found the foot in the vagina, where Dr. Harley had left it, and seized it, and tried to turn, but also found the uterus was so tightly moulded to the child that I could not complete the version; and as the fetal heart could not be heard, I determined to divide the neck. I placed the handle of Pajot's forceps on the neck without very much trouble, but failed to make the bullet roll, and had to pass my hand behind the neck till I met the instrument, when I drew down the bullet, and so got the cord into its place. I now passed a speculum as directed by Pajot, and proceeded to cut through the neck, but at the end of one minute and a half of the sawing motion the cord broke, and I feared the operation had failed. On examination, however, I found the greater part of the neck, including the spinal column divided, and I now tried to bring down the foot and effect version, which I did without any difficulty whatever, and the woman made a rapid and satisfactory recovery.[a]

This is an example of the second class of cases in which version cannot be effected; those in which the hand can be introduced and the foot seized, but the body of the child cannot be turned, I think we may say these are the cases in which evisceration is most difficult and dangerous, and decapitation is most easily performed. Certainly, when I contrast the safety, ease, and rapidity

[a] The child was exhibited at the Society, showing the head attached to the body by a small portion of skin and muscle.

with which the operation was performed with the difficulty I have myself experienced and seen occur with several experienced operators in performing evisceration, I am most favourably impressed with the value of the operation of decapitation.

On reflecting on the case, it seems to me that the operation might be very much simplified and also rendered safer, and still without infringing Pajot's principle of not multiplying instruments. The cord might be very easily carried round the child's neck by means of an elastic catheter, mounted on a firm stylet, or, what would be better still, mounted on a uterine sound. The firmness and broad handle of the sound would enable us to direct more easily the point of the catheter. The catheter should be a good deal curved; it should form about the fourth of a circle, whose diameter would be three inches. The first step of the operation is to hook this on the child's neck; then, the stylet being held steadily in its place, the catheter is to be run forward, when the curve of the stylet will cause it to pass round the neck till its point can be easily reached by the fingers, or may come into view. The cord may now be attached to the catheter, and drawn back with it, or it may have been previously passed through the catheter, and brought out at the eye of the instrument; so that it can be seized when the catheter comes within reach of the fingers. The ends of the cord are now drawn through a speculum, the catheter having been first removed, and the neck is divided as Pajot directs. If no speculum be at hand, the soft parts of the mother may, as Pajot suggests, be protected by the broad handles of two spoons introduced into the vagina, and used as retractors.

It will be seen that this suggestion differs from Heyerdahl's only in that the internal wire is held steady, and the outer tube is the part pushed forward. It is probable a soft elastic catheter would find its way more safely than the firm wire; but the chief object of the suggestion is that it does away with the necessity for a special instrument, which might not always be at hand when required.

The operation might be further improved by using an écraseur for dividing the neck, by which the accidental interruption of the operation by the breaking of the cord would be avoided, as well as the risk of injuring the soft parts of the mother by its friction. Once the cord is placed round the neck there would be no difficulty in attaching the chain of an écraseur to it, and drawing it into its place. The use of an écraseur invented by Dr. Ritchie

for this purpose was, according to Dr. Barnes, mentioned at a discussion at a late meeting of the Edinburgh Obstetrical Society by Dr. Keiller, but I have not been able to meet with a report of this discussion.

As soon as the neck has been divided, the body may be easily extracted by drawing down the arm, and if the uterus act well it will very probably expel the head; but if not, it may be seized with the forceps or cephalotribe. In using the cephalotribe in this case it will not be necessary to perforate in the first instance, because when pressure is applied the brain will escape through the foramen magnum; and, in avoiding the perforator, we avoid one great source of danger and difficulty in this part of the operation. But it is very probable that the best way of completing the operation would be to aim at the production of the condition that arose in the case under consideration from the accidental breaking of the cord—that is, to stop as soon as the spinal column is divided, and to leave the head attached to the body by a portion of the soft tissues of the neck; then to deliver by the foot, and let the head be drawn down still attached to the body. It is obvious that the division of the spinal column would do away with the difficulty of turning, and the tissues attached to the head would afford us a ready means for its extraction.

I feel that I should apologize for laying before the Society a suggestion for the performance of the operation which has never been tried; but in doing so I am only following the example of Scanzoni and Pajot, who at the time of publishing had not themselves put their suggestions in practice.

ART. XXVI.—*On a Case of Leucocythemia.* By HENRY EAMES, M.D., Dub., Physician to Mercer's Hospital; Joint Lecturer on Practice of Medicine and Pathology, Ledwich School, &c.[a]

THE malady which I have the honour to bring under the notice of this Society is of rather rare occurrence, and is one of those which owe their separate, and distinctive existence to modern research. In the year 1845 the disease known as leucocythemia, or excess of white cells in the blood, was nearly simultaneously described by Professors Bennett and Virchow; the former having preceded the latter in his description by six weeks. So far as we

[a] Read before the Medical Society of the College of Physicians.

can see at present, this is an idiopathic affection of the lymphatic system. It consists not merely in an increase in the number of the white cells, but in a proportionate decrease in the number of the red discs. Virchow has divided this disease, which he has named leukæmia, into two forms—one, the splenic; the other, the lymphatic form. Having mentioned that these two are often united, the spleen and the lymphatic glands being both enlarged, he gives rules for the distinction of the two by the morphological appearance of the white cells. In the splenic variety the cells being larger, and more perfectly developed, with one or more neuclei; in the lymphatic variety the cells are small, the neuclei large in proportion, generally single, granular, sharply defined and closely applied to the cell wall. Professor Bennett does not adhere to this division of the disease, believing it to be unnecessary. The distinguished Edinburgh Professor believes that the lymphatic glandular system, by which he means the spleen, thyroid, thymus, mesenteric, and lymphatic glands, with the pineal and pituitary bodies, secrete the blood corpuscles in the same manner as the testes secrete the spermatozoa, or the mammæ the milk. The exact mode in which the corpuscles are formed is by the production of molecules in the organic fluid; the successive aggregation and development of these constituting the higher formations. In the circulation, probably in the lungs, colour is added to them, and they become blood discs. Multitudes of free neuclei precipitated into the current are rapidly converted into red blood cells. The white cells circulate for a time as colourless corpuscles; but after a certain time their cell wall is dissolved, and their neuclei are also converted into red discs. In health the proportion between the white and red cells is about 1 to 400. But in leucocythemia the colourless cells are increased in number, whilst the free neuclei and the red discs are largely diminished; the reason being that the formation of red blood is checked in the lungs. Does, then, the essence of the disease consist in the abnormal development of cell walls around the otherwise free neuclei, hindering or impeding their transformation into red discs?

I shall now proceed to detail the case, and shall afterwards produce the individual who is affected with the disease, and afford to the members of the Society an opportunity of verifying our diagnosis by a microscopic examination of the blood, which, it may be mentioned, is the only certain means of diagnosing this diseased condition from other forms of glandular and splenic enlargement

Robert Williams, aged thirty-five, admitted into Mercer's Hospital, April 4, 1871. Is a stone-mason. Dates the commencement of his disease five years back. He had been out all day, the day being remarkably fine, so that he is certain that he did not catch cold. He eat his supper heartily, the meal differing not at all from his ordinary supper. Went to bed at 10 o'clock perfectly well. At midnight he awoke suffering from severe pain, which occupied the left hypochondriac and lumbar regions. It did not extend to the epigastric, umbilical, or iliac regions. The pain was of a burning character. He is sure that he had no rigors, thirst, or heat of surface. The pain was continuous. He consulted a medical practitioner, who told him he was passing a renal calculus, and prescribed an opiate and a warm bath. This treatment produced sleep, and complete relief from the pain for two hours. The pain then returned, and was just as violent as before. He again had a warm bath, and relief followed. After a few hours he was again attacked. This time the warm bath gave no relief. The pain continued for twenty-four hours without remission, and then disappeared gradually. There were no lithic acid deposits in his urine at this time. Feeling very weak after this attack, he consulted another medical gentleman, who told him, after having examined his urine, that his kidneys were diseased. He soon afterwards got better, and was able to resume his work, which he was able to continue till sixteen months since. However, during all this time he was in the habit of vomiting, the act not being accompanied by pain or forcing. It always occurred immediately after or during a meal. The matters vomited were never of a dark colour; they consisted merely of the food which he had taken. Sixteen months ago, in January, 1870, he had to work after hours, being at the time in a weakly condition. He perspired freely from slight causes, and going home one night, which was very chilly, he caught a severe cold. He had cough, with rusty-coloured sputa, and oppressed breathing. The cough went away in about three weeks; but he continued to lose strength and weight, and suffered from nocturnal perspirations. He had always been pale, but now his pallor was greatly increased. In April, 1870, he was advised to go to Trefriw, a place in Wales celebrated for its ferruginous waters. He had been there a fortnight when he was attacked with diarrhœa, unaccompanied by pain. He had ceased taking the water. The stools were then light-coloured and jelly-like. The diarrhœa was checked by mistura cretæ. He

came home feeling worse and weaker than when he left. He was obliged to remain in bed for a fortnight suffering from severe pain over the abdomen. His abdomen was now considerably swollen; his clothes would no longer meet on him. His medical attendant told him he had a tumour, ordered him a mixture, and to paint the tumour with tincture of iodine. His strength continued to decrease. He now went to Liverpool, and placed himself under the care of an eminent physician, who diagnosed a splenic tumour. He was ordered reduced iron and quinine, the tumour to be rubbed over with iodide of lead ointment. He remained under treatment for six weeks without any alteration in his condition. In October he went to an hospital in London. His blood was here examined under the microscope, and pronounced leucocythemic. At this time he had pain over the abdomen, diarrhœa, prostration, and night sweats. He could not lie on his back for any length of time. He remained in London one month, having, whilst there, become a little stronger. Since that time he has become gradually weaker. He has occasional attacks of diarrhœa, and vomits also, but not so frequently as at first. He suffers from dyspnœa, which has increased *pari passu* with the splenic enlargement. Four or five months ago he was affected with a frequent desire to pass water, and the urine dribbled after micturition. This has increased much of late. He never had any fever or ague; never lived in a marshy country, nor has he ever been abroad. He was always a total abstainer till this disease began.

His condition when examined was as follows:—His skin was of a yellowish hue, the conjunctivæ being clear in colour. His lungs and heart were healthy. No murmur in his veins or arteries. Abdomen greatly distended; girth round umbilicus thirty-six inches; midway betwen umbilicus and ensiform cartilage thirty-eight inches. A tumour, evidently splenic, extends two inches to the right of the umbilicus, and touches the symphisis pubis. It can also be plainly felt behind at the left side of the lumbar spine. The tumour is extremely hard. There is no splenic murmur. There are no glandular enlargements. The percussion dulness of the liver is not increased. There is no ascites. His feet and legs are slightly swollen. If he spends the day out of bed they become considerably œdematous in the evening. He suffers from diarrhœa, passing very liquid stools. The amount of urine passed in the twenty-four hours is thirty-five ounces. The following analysis has been made for me by my distinguished friend and colleague, Dr. Cameron:—

Two specimens taken on different days were examined :

No. 1. Sp. gr. 1015·7.
Slight acid reaction.
Of 100 grs. of urine the solid residue = 4·31.
The amount of urea = 1·06.

No. 2. Sp. gr. = 1016·0.
Slight acid reaction.
100 parts of urine afforded by evaporation 4·09 parts of solid residue, including 1·08 parts of urea.

Thus it will be seen that 1,000 parts of urine contain only 10·6 parts of urea; the lowest healthy standard being about 15 parts in the 1,000. The solid matter of healthy urine contains, according to Dr. Beale, half its weight of pure urea; this urine contains only one-fourth. Since the amount of urine passed daily is, as stated, thirty-five fl. oz., the daily amount of urea excreted is 153·125 grs. The average amount for a healthy man of our patient's size, on full diet, is about 400 grains in the 24 hours.

It is therefore plain that in leucocythemia the amount of urea excreted is very much diminished.

It is also noteworthy that a specimen of the urine, after remaining for a week in an open vessel, had but an extremely feeble ammoniacal odour.

Some blood drawn from his finger, and examined under the microscope, exhibited a large excess in the amount of white corpuscles. From many careful examinations the relative proportion between the red and white cells was estimated at three of the former to one of the latter. The white corpuscles are large, and nearly all of them multinuclear, thus bearing out Virchow's observations on splenic leucocythemia.

His temperature is normal. His mental powers are unimpaired. He suffers much from weakness, being easily fatigued. He has no tendency to fainting fits.

My friend, Dr. Fitzgerald, kindly examined my patient with the ophthalmoscope. He states that the fundus oculi presents the orange tint which is commonly found. The margin of the right optic disc is very ill defined and is striated. The retinal vessels are of rather fainter hue than normal. There were, however, none of those white spots, or stripes along the sides of the vessels which have been generally found in this disease, and which are supposed to depend on the agglomeration of white blood cells escaped from the vessels. There were no hæmorrhagic effusions. The left optic disc was well defined.

During the time he has been in hospital he has had a fair appetite. The treatment adopted has been, at first, citrate of iron and quinine in 15 grain doses. Changed, after a little time, to tincture of the perchloride of iron in 15 m. doses three times a day. He is frequently attacked with diarrhœa. This is temporarily checked by decoction of logwood with tincture of opium in 7 minim doses. He has had one severe attack of pain in the left lumbar region, which was relieved by the hypodermic injection of morphia. He affirms that he feels improved since his arrival in Ireland. He no longer vomits, and his muscular strength is greater.

ART. XXVII.—*On the Treatment of Psoriasis by Balsam of Copaiba.* By HENRY SAMUEL PURDON, M.D., L.R.C.P.; Physician to the General Hospital and to the Hospital for Diseases of the Skin, Belfast.

THE following short communication is intended to call the attention of the profession to an excellent remedy for psoriasis, viz.:—The balsam of copaiba, a drug, introduced some three years since into the list of cutaneous therapeutics, by M. Hardy, of St. Louis' Hospital. The way it was found to possess special virtues for curing psoriasis may be reported as follows:—A patient was admitted into hospital suffering from gonorrhœa and psoriasis; as it was necessary to have the former complaint well before commencing treatment for the latter, he was ordered copaiba, the result being that not only the gonorrhœa, but also the psoriasis rapidly disappeared.

Copaiba, is an oleo-resin, acting medicinally as a powerful stimulant to mucous membranes, exciting readily a new action in those structures when diseased, probably not only by both actual contact, as in the blood or urine, but also through means of the nervous system; hence copaiba is occasionally prescribed in some forms of bronchitis, affections of the bladder, urethra, and rectum.

We know that erythema and urticaria are frequently caused by the administration of copaiba, the last mentioned disease, being now recognized as a "cutaneous neurosis."[a] I wish, however, first to say a few words on psoriasis, which is a cutaneous disease, "peculiar to itself," difficult to permanently cure, and prone to relapses. In

[a] Hardy has described a case of pemphigus occurring during the administration of copaiba. Hebra has described a case of urticaria in which several of the pomphi passed into bullæ.

its early stage *a patch* of psoriasis presents a more or less congested appearance, very slightly raised above the level of the surrounding skin, having no tendency to spread by continuity of surface, but gaining in extent, through the joining together of isolated spots of either psoriasis guttata, or punctata. In the chronic stage, the subcutaneous cellular tissue is occasionally infiltrated, the disease under notice being regarded especially by German Dermatologists as an inflammatory condition of the outer-layer of the corium and papillary bodies, accompanied by cell-proliferation, giving rise to well-marked enlargement of the papilla. We, moreover, know that nerve-irritation can induce rapid cell-formation and metamorphosis. If debility be likewise present, there is no doubt diminished control over the tissues, consequently the nature and intensity of the exciting cause must be allowed to exert more or less influence upon cell-growth, and the well-known silvery-looking scales characteristic of psoriasis, situated upon a red and infiltrated corium, are caused by increased desquamation of the epidermis; hence in treating the chronic stage, various local stimulating applications are used, as for instance, tar, oil of cade, creosote, carbolic acid, preparations of potash, &c.; likewise we give internally "stimulants" which is only another word for a division of tonics, as arsenic, a pure nerve tonic, quinine, &c. But to return from this digression. During the last eight months I have had under treatment at the Belfast Hospital for diseases of the skin an unusually large number of cases of psoriasis. The opportunity thus offered of grouping together a certain number of cases, and of investigating the different effects of certain remedies, as arsenic, carbolic acid, hypophosphite of soda, cod-liver oil, quinine, balsam of copaiba, &c. In some cases local treatment was also ordered. Without, however, entering into details regarding the different modes of treatment, or extending this paper with the recording of cases, I may be permitted to say that the treatment of psoriasis, *when no acute symptoms were present*, by large doses of balsam of copaiba, given with a little liquor potassa, mucilage, and water, has been highly gratifying, especially in cases where it has produced extensive urticaria; indeed the dose should be increased till the latter is established. I have also been able to discharge the patients sooner by means of the balsam treatment than by any other, nor have any of them as yet had a relapse, which in psoriasis is generally the rule, but, of course, the time is too short to speak definitely on this point.

Can any satisfactory reasons be given for the success of this plan

of treatment? I am inclined to think that it acts somewhat as follows:—We know that urticaria is a cutaneous affection, generally due to irritation of the mucous membranes either of the stomach, or of the uterus, in the former instance presided over by the solar plexus, in the latter by the uterine nerves, the cutaneous manifestations being merely symptomatic, as indeed nearly all skin diseases in the first instance are, afterwards becoming local affections, accompanied to a certain extent by alteration of structure in the part attacked. Now, if psoriasis is an inflammatory disease of the skin, we have by prescribing copaiba, either as balsam or as oil, the power in most instances of producing erythema, often associated with urticaria, a condition of things analogous to an acute inflammation of the skin from a definite, or specific cause; indeed the erythema may be regarded as inflammatory, at any rate it is often the first stage of inflammation. Now, according to John Hunter, two inflammations cannot co-exist, the most recent usually destroys the other, as is observed in what has been called the abortive treatment of gonorrhœa, by a strong injection of nitrate of silver; again the wheals produced on the skin by copaiba are nearly bloodless, hence absorption can and does more readily take place, the activity of absorption being unusually in an inverse ratio to that of the circulation. The serous engorgement must also be got rid of; this condition just described being favourable to the removal of the hypertrophied papillæ and infiltrated state of the corium, due to a "bloated" condition of their cell elements as existing in psoriasis. I am the more inclined to this view by the fact of one variety of urticaria being occasionally exhibited, called *lichen urticatus*, generally occurring during the course of acute febrile diseases, and we have a good deal of febrile disturbance of the system when the stomach is "upset" by copaiba. The primary lesion of psoriasis is of a more or less papular character, if I may so express myself; hence the psoriasis punctata and guttata are acknowledged by Dermatologists to be the commencing varieties of that disease. Bateman was aware of this when he tells us that Willan's three first species of lichen often lose the papular form and "occasionally pass into psoriasis."

Balsam of copaiba then by causing derangement of the stomach, of a mere temporary character, inducing irritation that is conveyed to the solar plexus, the great centre of the sympathetic system

[a] Urticaria, both acute and chronic, seems to be a morbid condition depending on reflex sensation often due to uterine or ovarian irritation. See a paper on Chronic Urticaria, by Dr. Fox of Bristol, Journal of Cutaneous Medicine, December, 1870.

presiding over organic life, and consequently nutrition, by this means exerts most probably what I may call a reflex influence upon the nutrition of the skin, the nerves presiding over the part or parts attacked, are awakened to a state of intense excitement, as may be proved by the formation of wheals, due to vaso-motor nerve spasm in the first instance and the influence exercised by such local disturbance on the cutaneous nerves causes the normal processes of textural life to be improperly performed.

The following note of one of my cases is interesting. The patient, a girl aged 15, who never had rheumatism, employed at a sewing machine, catamenia regular, no family history of chorea, and enjoyed good health till about ten months since, when psoriasis appeared first on knees and elbows. Three months ago she came under treatment. I commenced the copaiba in half drachm doses thrice daily, and in three weeks the eruption (although no urticaria or erythema was produced) began to fade. About a week after this date symptoms of chorea exhibited themselves, the facial muscles first attracting attention, and in another fortnight the disease was fully developed, accompanied by a dynamic cardiac murmur at left apex. The copaiba was discontinued on the first symptom of chorea becoming manifested, and wine of iron with Fowler's solution substituted. Was this a case of what Trousseau, in writing regarding the influence of skin diseases on development of chorea, called "the mutual transformation of diathesis?" or was it due to debility, so common a cause of not only psoriasis but also of chorea. The only author that I know of who mentions the combination of psoriasis with chorea is Dr. Handfield Jones. At the present time (April) I am happy to say my patient is quite well of both complaints.

ART. XXVIII.—*Diarrhœa, with Green Stools, in Infants.*[a] By STEPHEN M. MACSWINEY, M.D., L.K., & Q.C.P. Physician to Jervis-street Hospital. Lecturer on Medical Jurisprudence, Catholic University.

SOME apology is due from me to the Society for asking to occupy the time of members upon so seemingly simple and common-place a subject as that whose title heads this case. But, in fact, the matter dealt with is related to a by no means unimportant inquiry. The ailments of newly-born children have special claims upon the consideration and care of the medical attendant. The little

[a] Read before the Obstetrical Society.

patients themselves are so helpless and unable to explain their symptoms—the affections of infants are, in many instances, so peculiar, and confined to their period of life;—those sicknesses which they may have in common with grown-up people are, out of all proportion, so much more dangerous to life than the corresponding diseases in adults; and the trembling anxiety of the parents is so great and so appealingly expressed to the physician respecting their sick baby, that an unusual interest must, of necessity, be felt by him in the condition of the little sufferer.

It is to a morbid condition, sometimes present in the alvine discharges of sick children, that I desire to draw attention this evening.

Every physician who is in the habit of seeing much of the illnesses of infants is aware that a symptom sometimes arises which experience has taught him he must regard with particular interest and care. And his anxious attention is aroused and fixed upon this symptom, not only because he himself is well aware that it often denotes a state of ill-health in the infant, fraught with danger to its life, but also because it is a symptom which tradition and experience has stamped in the minds of parents and the general public with a character of much gravity and danger—I allude to the occurrence and persistent continuance of "green stools," so frequent in the progress of the diarrhœas of newly-born children, and also, not rarely manifested when a child is suffering from the irritative fever attendant upon a "teething fit." The following case, whose course I observed with more than ordinary attention and interest, affords a good example of the morbid state to which I refer. A female infant was born November 18, 1870. In healthful appearance and size she was quite up to the average of newly-born children. It was expected that the mother might be able to nurse her after three or four days, and in the meantime she got a little sweetened and diluted cow's milk. After the meconium had been all got rid of, the bowel discharges were noticed not to show any appearance of yellow colour; but to be decidedly greenish, and much too frequently passed. At the fourth day she got her mother's breast, and as this was insufficient in quantity, the deficit was sought to be made up by bottle-feeding with cow's milk, reduced and sweetened in the usual way. This plan was persevered in for a fortnight, but the result was unsatisfactory. Only once during this time was there a healthy-looking, yellow evacuation—the others, which were far too numerous, were stringy

and green. At the same time, the infant was manifestly getting thin and looking delicate. In fact, the mother's breast, from the absence of sufficient milk, and the condition of her general health, was useless to the child. It was therefore decided to substitute a wet nurse This was accordingly done; the child got nothing except the new breast milk, but, instead of improving, she now became decidedly worse. The relaxation of the bowels passed into diarrhœa, and superadded to the characters of stringiness and green colour in the discharges was the additional one that they were now curdy. This was allowed to go on for a week, when it became clearly apparent, on the supposition that the breast milk was in fault, that this wet nurse must not be retained. Another wet nurse was, therefore engaged, but again the change, instead of being followed by favourable results, seemed only to produce an exactly opposite effect. Matters rapidly went from bad to worse; there would now be from ten to fourteen napkins soiled in the twenty-four hours, all the discharges presenting the same remarkable characters of bright pea-green colour, numerous lumps of curd being scattered through them, and exhaling a sour but not fæculent odour. When this state had continued for some time, it became manifest that the frequent and unhealthy discharges were beginning to tell seriously on the child's constitutional strength, and grave apprehensions arose in our minds as to her ultimate recovery. It was determined, as a last resort, to change the wet nurse once more, and strenuous efforts were made to procure another. It so chanced, however, that unexpected difficulties were experienced in accomplishing this, although the usual sources were applied to. In the meantime, whilst seeking for another wet nurse, a slight improvement (to be followed, however, by a speedy relapse) would now sometimes occur, and raise hopes, which were frequently disappointed, that the affection was coming to an end. Finally, at the expiration of ten weeks, this wet nurse being, of necessity, retained, the bowel discharges had assumed a healthier character; though still loose and frequent, they were less so than they so long had been; and the child, which had been much wasted, began again to put up a little flesh and look less unhealthy. This improvement has been maintained. At the present time she is taking the breast of Nurse No. 3, calling the child's mother Nurse No. 1. This nurse is the same who we wished to part with, and whilst using whose milk the child became more alarmingly ill than she had before been. She is completely digesting the milk

now, and to all appearance is thriving remarkably well. To complete the medical history of this little patient, two other circumstances remain to be mentioned—namely, that no vomiting ever occurred throughout the duration of the sickness, and that the child did not, seemingly, suffer from pain during the entire progress of the case.

In the report just read I have not alluded to the drug-therapeutics had recourse to in the case. They may be comprised in a small compass. The medicinal treatment of the ailments of infants does not admit of being extensive or heroic. The remedies used must be simple and few in number; the doses small and not too frequently repeated; the effects watched with care and anxiety. However, the approved remedies were used in this case, cautiously and under the direction of the best advice, with the effect of creating in my mind a strong tendency to believe that drugs are quite powerless to control the affection in infants which I describe. All medicines and dietetic combinations appeared to be utterly valueless, if not, indeed, positively injurious, with two exceptions. I think the child was somewhat benefited by the use of small doses of the *Liquor Calcis Saccharatus*, recommended by Dr. M'Clintock, and by the use of a few teaspoonfuls at a time of a food consisting of sago boiled on water, with milk and a few drops of brandy added, advised by Dr. Cronyn. The following are the three questions of some interest which appear to me to arise out of a consideration of this case. A. What is the nature and constitution of these green stools in infants? B. Are they due to inherent delicacy in the child? Or C. Are they caused by unsuitable nutriment, howsoever applied? With regard to the first question, I believe it is altogether unanswered by men of science up to the present time, and I certainly am not prepared to solve the problem. I cannot anywhere find that a precise scientific opinion has been arrived at by chemists, after careful analysis and investigation into the nature of this green colour in the dejections. These coloured discharges almost always co-exist with diarrhœa. They are usually squirted from the bowel with considerable force, the ejection being accompanied by a loud noise. In most instances a quantity of stringy and green-coloured mucus is commingled with the motions proper. These latter possess no odour of fæces, but have a penetrating sour smell. Indeed, the appearance of these motions is very characteristic. The colour varies from very light pea to a very dark grass-green tint. Generally they are

not green when passed, but rapidly become so upon exposure to air. Frequently, however, as I have many times witnessed in the case just related, and as was seen by Betz in his cases, they are already of a deep green colour when escaping from the rectum. These discharges resemble very much the motions passed after a *mercurial* has been given to an infant. Interspersed through the evacuations are to be seen, usually, numerous firm white lumps or specks. These are composed of the undigested casein of the milk, and well denote the great disturbance of the principal functions of the stomach which exists. Furthermore, occasionally, in severe attacks where this symptom is present, drops of blood—and sometimes even a considerable quantity of blood—may be observed poured out in the motions. As I have just said, I believe it has not, as yet, been established to what cause the green colour of these discharges is due. Nurses call them "griped, bilious motions," and they thus express the popular—if not, also, the correct—explanation. In 1853 J. Vogel, of Giessen, stated that he had detected in the maternal milk certain *infusoria* (vibrio bacillus) to the presence of which he ascribed the occurrence of diarrhœa and green stools in the infant. Gibb has confirmed this observation, so far as the *infusoria* are concerned. He has discovered two genera of animalcules in the milk of unhealthy women, *vibriones* and *monads*. The two varieties are never found together in the same woman. Vogel supposed them to be the results of fermentation, and Gibb found much sugar in the milk, and that it underwent rapid acid fermentation out of the gland. It appeared to result, however, from the investigations of Golding Bird, that the green colour of these stools was due to altered blood. Betz (Mem. a. d. Prax 7 Schomidt, Vol. cviii., p. 202) rejects this opinion of Golding Bird and Brednar; but I cannot find that he substitutes any theory of his own. Other inquirers have referred this green colour to the action of abnormally secreted intestinal acids upon the pigmentary matter of the bile. But I think the question arises here, Is there an excess, or even a vitiated condition of bile in these cases at all? or is there, rather, possibly, an inaction of the liver and a total absence of bile from the intestines? Bile is a very powerful antiseptic; its absence, of whole or in part, is followed by decomposition in the intestinal contents. But in the diarrhœas of children, with green stools, there is much accumulation of gases in the bowels, and their contents are more or less putrid in smell. The green stools

caused by a mercurial (the "calomel stools" of Kraus) have received a variety of explanations. It was the custom to refer them to an action upon the bile; but recent investigations tend to disprove this view. Kraus referred them to the operation of calomel contained in the alimentary canal; whilst Zeller thinks they depend upon alterations produced in the condition of the blood. I believe the whole question of the nature of the pigmentary matter in animal bodies, more particularly that of the healthy and morbid intestinal secretions of the bile and of the urine, is as yet in a very unsatisfactory and imperfect condition. To the other difficulties in the way of determining the mode of production of the green colour alluded to must be added that arising out of the undoubted facts that the stools are sometimes passed yellow, soon changing to green; whereas, at other times, they are perfectly green whilst yet in the rectum. This further complicates the already unexplained problem. I have no pretensions to attempt its explanation, and I leave the subject here now, merely commending it as a fit one for scientific investigation at the hands of our analytical chemists.

B. In reply to the second question, I would beg leave to suggest that the origin of this affection is often existent in the baby's own organization. The presence of the undigested casein in the motions of the child is evidence, no doubt, that the diet is unsuitable. But then, it may fairly be asked, is the original error or defect in the nurse, or in the child itself? I cannot help being of opinion that it is much more frequently in the child than is commonly taught or supposed. I think we may confidently affirm that an infant may be in such a state of delicacy at birth as will render it liable to be attacked by diarrhœa, with green stools, excited by agents that would have no effect upon a healthy child. There may be, and there probably often is, a congenital functional digestive weakness in the child, in such cases as these, which will always cause to be left an undigested surplus capable of exciting constant alimentary irritation. This would induce an alteration in the glandular secretions of the intestines. Irritation of the mucous glands would ensue, stimulating the mucous membrane to increased secretion. In fact, a true gastro-intestinal catarrh would be induced. After some time, acid fermentations of the food would result, the acids thus generated acting—perhaps on the bili-verdin, as previously intimated; perhaps in some other hitherto unexplained manner—as the principal agents in the

production of the green colour. An argument in favour of the view that the child, not the nurse, is in fault is found in the fact, well known to those who see much of the illnesses of young children, that bottle-fed infants, or infants that, having been weaned, are fed upon bread and milk, or such like food, are frequently attacked with diarrhœa and green stools, which can no longer be referred to the influence of breast milk. And these same children will, after a longer or shorter time, return to health, the diet remaining substantially the same all through the illness. Moreover, in the case I have to-night reported, the infant got well whilst taking, of necessity, the milk of that nurse whose breast appeared, for a long time, to disagree most of all with her, and who only escaped having her services dispensed with by the accident that another wet nurse could not, at the time, be procured. The absence of vomiting in this case would appear, also, to lend support to the view that the digestive system of the child was the source of the ill-health, and that the origin of the irritation was in the intestines; whereas, if the breast milk had been disagreeing, we might expect to find that the deleterious effects would be announced at once by the disturbance it would set up in the stomach, and by the presence of some amount of vomiting or regurgitation.

C. I now come to the third of the questions which I have proposed; its discussion involves considerations of deep importance to the well-being of infants. It is not necessary to urge here how essential to the medical man are clear views upon the question of the proper food of infants. A knowledge of the proper nourishment of children is a fundamental necessity for their successful treatment when sick. The newly-born infant, in particular, demands the utmost care and attention in regard to its nutriment. All authorities are agreed as to the mother's milk being, when it is of normal, healthy condition, the best food for the baby. But experience teaches us that a nursing mother's milk may, sometimes, certainly disagree with the child. The lacteal secretion is known to vary in composition very considerably in different women, and is, occasionally, so abnormal in composition as to act most injuriously on the infant partaking of it. In infants the activity of the digestive organs is very great; their susceptibility to irritation very considerable; and these would predispose them to quickly manifest symptoms denoting the disturbance. These symptoms are referred chiefly to an increase in the quantity and

frequency, and an alteration in the objective characters of the evacuations from the bowels. Hence it is in this direction that we look for evidence, in a particular case, as to whether or not the milk is agreeing with the child.

Numerous investigators have examined human milk. Amongst these may be named, Simon, Lieberkuhn, Scherer, Vogel, Muter, Vernois, Becquerel. Human milk is alkaline; the mean of eighty-nine analysis showed it to have a specific gravity of 1032·67, and to be composed as follows, viz.:—

Water, . . .	889·08
Sugar of milk, . .	43·64
Casein and extractive, .	39·24
Butter, . , .	26·66
Incombustible salts, . .	1·38
	1·000

The investigations referred to have demonstrated the fact, alluded to above, that the variation in the composition of milk, from the standard of health, is occasionally very considerable. Thus there may be a great excess of the flesh-forming ingredient—the casein; in like manner the heat producers—fat and sugar—may be present in abnormal quantity—or the milk may be deficient in both or either of these substances. So also, the amount and proportion of the phosphate of lime, and the other salts normal to human milk is a variable, not a fixed quantity. In fine, the total quantity of milk secreted may be much under what is requisite for the nourishment of the child. From original defect in constitution the woman may be totally unable to produce healthy milk. Or, in the case of healthy women, many accidental circumstances may exert an injurious effect upon this secretion. Such are the use of certain kinds of food—partaking either of insufficient or too abundant nutriment; a sensitive and excitable temperament; sudden emotions—as passion, fright, fear, despondency, and others that might be named. I may barely allude, also, to the very general—I know not if well founded—belief that menstruation during lactation exerts a deleterious influence on human milk.

That the particular breast milk is disagreeing with a child is often apparent enough. The state of the stools in these cases will testify to the quality of the milk, whilst the urine passed gives a good measure of its quantity. If, in addition, the child should appear thin, and not "thriving," we may conclude the breast milk

is bad. But when it is desired to engage a wet nurse, medical men are expected to determine in every case, by examination of the woman, if she be fit for that duty. Can this ordinarily be done by them? I am persuaded it cannot, at least in very many instances. I maintain that the common method pursued by physicians when examining into the suitability of a woman to act as a wet nurse is wholly insufficient to accomplish the object in view. The procedure is usually something like the following:—In the first place, the state of the woman's general health, appearance, family history, &c., is inquired into. Then some of the breast milk is procured; is tested, perhaps, with litmus and turmeric papers, and is set aside to stand for some hours. It is, I presume, never tasted. When next examined some cream will be found on the surface, and an inspection of this, and of the underlying fluid, by the doctor, is supposed to be capable of supplying the information desired. Nothing can be more defective in the means of attaining the end required than this process. It leaves undetermined the specific gravity of the fluid; it ignores the facts that there is considerable difference between different specimens of healthy milk in the quantity of butter they contain; that no knowledge can be obtained by this method of the amount of casein and albumen, or of sugar present; that the proportion, moreover, in which the various salts, highly important as they are, exist in it cannot be determined; nor can the amount of water be ascertained. I beg to conclude these remarks with the following summary:—1st. The cause of the green colour in the stools of infants has not been established. 2nd. It may be doubted if the presence of bile is necessary to the production of this colour. 3rd. These stools are often due to an inherent delicacy in the child. 4th. Human milk varies much in composition, and is occasionally injurious to the infant fed on it. 5th. A ready method of testing the quality of breast milk is still a desideratum by physicians.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Notes and Recollections of an Ambulance Surgeon, being an Account of Work done under the Red Cross, during the Campaign of 1870. By Wm. MacCormac, F.R.C.S., M.A., M.R.I.A.; Assistant Surgeon to St. Thomas's Hospital; Consulting Surgeon to the General Hospital, Belfast; and Member of Senate of the Queen's University.

What we Observed during a Visit to the Seat of War in 1870. By Charles Orton, L.R.C.P., Edinburgh; Medical Officer to the North Staffordshire Infirmary; and Wm. Dunnett Spanton, Surgeon to the North Staffordshire Infirmary.

These notes of the recent campaign, though both in the greater part reprints of communications to the medical journals, will be read with much interest even by those who before read them in part in their original form. To those who have seen no part of the original letters they will afford accurate information of the nature and character of the work done by volunteer ambulances. To all interested in the medical history of the war, their value is great, for they are the earliest reliable instalments of that history published in this country. Mr. MacCormac's report gives us the account of the work of the Anglo-American Ambulance at Sedan, from the 28th August to the end of the campaign. The report of Messrs. Orton and Spanton gives a sketch, a very hasty one no doubt but still one full of interest, of the work of the ambulances of several countries Mr. MacCormac's among the number drawn by visitors well acquainted with the practical working of hospitals. Perhaps they would have done better had they confined their criticisms to medical subjects, for some, at least, of their observations on general subjects border on Cockneyism; for instance, when they tell their readers, that "we found Prussians, officers and men, very intelligent, courteous, and well-informed," one is forced to conclude that they had set out on their travels prejudiced against the German

race, as being of a type of humanity inferior to that of their British readers. Mr. MacCormac's account of the working of his ambulance clears him and his senior colleagues, as far as we can judge, from the charge made against them by Messrs. Orton and Spanton, of having, to the detriment of the wounded, reserved to themselves the performance of operations which might have been performed by the junior members of the staff of surgeons engaged with them, for we find that the entire staff had far more work to do than their united efforts could accomplish, and that in the performance of it, the order established seemed to be a necessity. In such matters a division of labour is clearly essential, and certainly if all were operators in chief the no less necessary secondary details would have been left undone. Mr. MacCormac, as we would expect from one who has attained a well-merited eminence as a surgeon, does not, in the brief account of his practice at Sedan, either suppress the merits of his colleagues, or put his own name unjustly forward. We wonder only in reading his work that any record of cases or operations could have been kept while excess of work, much of it accomplished under fire, seems to have been the rule, especially at the times when most of the important operations were performed.

Whoever reads Mr. MacCormac's book will admit that no little energy and courage is required for the duties of a volunteer ambulance surgeon, if his ambulance ever has a real existence. There seems to have been more than one sham ambulance engaged in the present war. The account published by Messrs. Orton and Spanton, of one of them established at Bingen, well away from the sound of battle, with a staff of no less than seventeen surgeons and dressers to attend two hundred patients, suggests the idea that there is such a game as playing at ambulances as well as at soldiers. Nature seems to have visited this institution with a sudden and violent punishment, for on October 30, 1870, "amid a storm unparalleled for its violence, the whole hospital was swept off the face of the earth." While thus objecting to humbug, nature was careful of the patients, for we read in the same account, "they all lived through it."

The results of military surgery in the field are far from encouraging, as the experience of all wars has amply proved, and the past campaign so far as our authority report it are no exception to the rule. This want of a success proportionate to the efforts made must tend much to dishearten even the most enthusiastic and patient. Mr. MacCormac expresses this depressing feature of

military surgery in his account of his interview with Stromeyer, by quoting what seems to have been the chief theme of their conversation, as follows:

"We afterwards had a long conversation together, in the course of which he asked me if I had yet arrived at his conclusion that the practice of military surgery was unsatisfactory in the extreme. He said he was now passing through his third campaign, and the longer he lived the more deeply was he alive to the seemingly unavoidable evils which attend the practice of surgery during war."

We do not at present enter into an examination of the statistics recorded by Mr. MacCormac, as we hope by and by to consider them with such others as may be published from the surgeons engaged in similar work, which have not as yet reached us. We will merely conclude this short notice of his work by recommending it to our readers as well worth a careful study, and congratulate its author on his success, and on the just acknowledgment of it expressed by the public in his recent election to St. Thomas's Hospital.

A System of Surgery, by Various Authors. Edited by T. HOLMES, M.A., Cantab. Second Edition in Five Volumes. Vols. III. and IV.

THE third and fourth volumes of the second edition of Holmes' *System of Surgery*, like the two previous volumes, are, in many respects, a great improvement on the previous edition. The several articles have been carefully revised, and rendered in their respective branches, as far as possible, complete chronicles of the progress of surgery during the last ten years. The illustrations introduced in the present edition add much to its usefulness, while its bulk is not unnecessarily increased, nor are they in number or vividness of colouring such as to bring the work into the class of surgical picture books so prevalent at present.

In Vol. III. the articles possessing most interest for readers familiar with the first edition are Mr. Holmes's paper on Aneurism, and Mr. Lockhart Clarke's on the Affections of the Muscular System. Mr. Holmes has had in the subject of his paper a great advantage over his fellow authors in having a subject to work on, which, while it is inferior to none in importance, or the extent of its literature, is still constantly enlarging, and still constantly baffles the skill alike of physician and surgeon, both in diagnosis

and treatment. The more recent modes of treatment—digital pressure, manipulation, and the various modes of forming foreign bodies within, or introducing them into aneurismal sacs—are fully described, and the latest cases are recorded in which the various methods have been tried. Referring to the latest proposed method—that of introducing fine iron wire (twenty-six yards) into the sac of thoracic aneurism, with a view to the coagulation of fibrin on the foreign body—Mr. Holmes writes as follows:—

"I have thought it right to refer to this case somewhat at length as illustrating the latest of many propositions for the treatment of aneurism incurable by the ordinary methods; but I do not find myself able to pronounce a decided opinion as to whether the process is less or more dangerous, and less or more likely to succeed than galvano-puncture, which is the only other available method in such cases. It must be remembered, on the one hand, that it has only been tried once, that a very large quantity, probably too large a quantity, of wire was used; and that a very considerable deposit of adherent fibrin was produced, accompanied by great temporary amelioration of the symptoms. These considerations might lead one to hope that in a future trial, with additional precautions, better success might be obtained."

Mr. Holmes thus expresses his estimation of the comparative merits of galvano-puncture, and simple acupuncture:—

"In my opinion, as far as I am able to judge from the few cases recorded, galvano-puncture is a less hazardous measure, and more likely to prove successful, than acupuncture without galvanism."

Dr. Lockhart Clarke's paper on Affections of the Muscular System is in part a reprint of Mr. Tatum's paper, published in the previous edition; but on some of the most important diseases of the muscular system Dr. Clarke has published brief accounts of the results of modern research into pathological changes observed in both the muscular and nervous system in these diseases. Dr. Clarke's authority in this branch of pathology is certain to command the attention of the readers of the present edition of *Holmes' Surgery*, and we are certain that his paper will not disappoint this attention. While the author has had ample opportunity for enlarging on the details of microscopic anatomy, he has spared his readers the infliction of reading any long and (as with most microscopists) unintelligible description of pathological changes. Dr. Clarke has briefly stated the facts in plain language, and in our

opinion, set an excellent example to modern writers on microscopy. Dr. Clarke thus sums up the knowledge of the present day on the subject of the essential changes in cases of progressive muscular atrophy.

"In 1861 I discovered in the spinal cord, removed from a well-marked case of progressive muscular atrophy, numerous lesions of the grey substance, consisting chiefly of areas of what I call granular and fluid disintegration. In several other cases which I subsequently with similar alterations of some of the white columns, with dilatation of the blood vessels, or atrophy of the nerve cells, and of the anterior roots of the nerves. Since my first publication on this subject, several German and French pathologists, by means of improved methods of observation, have obtained similar results."

Of the two forms of muscular atrophy, which are peculiar to the period of childhood, viz., the atrophy which follows infantile paralysis, and degeneration of muscle, with apparent hypertrophy, Dr. Clarke gives brief accounts. Of the pathology of the latter disease, first described by Duchenne in 1858, we find the following account:—

"Nothing abnormal could be discovered in either the nervous or vascular systems. Eulenburg and Cohnheim found the electro-muscular contractibility everywhere perfect. To the touch the muscles gave the sensation of a doughy and inelastic mass. They were marked with stripes of a yellow or yellowish-white appearance. At certain points they could not be distinguished by the naked eye from the subcutaneous adipose tissue. There was also great hypertrophy of connective tissue between the muscular fibres, which were frequently much reduced in diameter, although they were not otherwise much altered.

"Duchenne and Ordonez found the striæ on the fibres very much finer than usual, and semi-transparent. The same appearance, together with an increase of connective tissue, and large collections of fat cells, were also observed in one case by myself."

Vol. IV. also contains two important papers by Dr. Lockhart Clarke, on the nervous system, as well as a paper by Dr. Brown-Séquard on the remote consequences of nerve lesions. These papers will be found of much use, as references are given to the leading cases recorded which bear on the subjects of disease and injury of nerves. Dr. Clarke's paper on locomotor ataxy is an excellent summary of our knowledge of this affection. Dr. Brown-Séquard's paper loses much in value by the repeated references to

the author's own publications, and from the fact of the article being rather a condensed argument in favour of the author's special views on nerve physiology than a practical treatise for the guidance of the practitioner who may refer to *Holmes' Surgery* as a text-book in cases of difficulty.

Mr. Salter's paper on the teeth, which has been carefully revised and illustrated, contains much practical information. In it we find a section devoted to the subject of odontomes, or tooth tumours, which contains new and interesting matter. These tumours are divided into—(*a*) Enamel nodules, or submerged cusps on tooth fangs; (*b*) Exostoses; (*c*) Hypertrophied, dilated fangs; (*d*) Dentine excrescences; (*e*) Warty teeth. Of each of these classes a careful description and illustration is given. This paper, as well as that of Mr. Durham's on the osseous tumours of the nasal fossæ, and of the sinuses of the face, will be read with much interest by those engaged in the study of the more obscure forms of tumour of the head and face.

Mr. Pollock's paper on the diseases of the mouth, pharynx, and œsophagus, contains the following acknowledgment of the important advance made in late years by the use of chloroform in this branch of surgery:—

"Mr. Thomas Smith was, we believe, the first to introduce the use of chloroform in operations on the palate in children, and he has entirely proved that, under its administration, clefts of the palate may be readily and successfully operated on in childhood at a very early age."

When one reflects on the great advantage such early operations possess in preventing the child from acquiring the faulty speech, the necessary result of cleft palate, this improvement in surgery will readily be admitted to be great. In the former edition of *Holmes' Surgery*, Mr. Pollock thus writes:—

"For many reasons the age of fifteen appears to be the earliest period for operation; often a later period is desirable."

Mr. Smith has disproved the statement "that chloroform is of no assistance, but inconvenient in its operation." We can speak from personal experience of the advantages of chloroform in this operation, and of the practicability of the operation as proposed by Mr. Smith.

An Introduction to the Osteology of the Mammalia. By William Henry Flower, F.R.S., F.R.C.S.; Hunterian Professor of Comparative Anatomy and Physiology, R.C.S., London, and Conservator of the Museum of the College. London: Macmillan and Co. 1870. Pp. 344.

A Manual of Zoology for the Use of Students. By Henry Alleyne Nicholson, M.D., D.Sc., &c. Blackwood and Sons. 1870. Pp. 620.

General Outline of the Organization of the Animal Kingdom and Manual of Comparative Anatomy. By Thomas Rymer Jones, F.R.S. Fourth edition. London: Van. Voorst. 1871. Pp. 886.

It is somewhat singular that there does not exist in the English language any text-book of zoology or comparative anatomy that a teacher can with perfect confidence recommend to his pupils, although no country can boast of such a number of earnest workers in this branch of natural science. We have many good monographs and works on special departments of science, but not one even passable on the general principles and outlines of zoology. Such being the case it is with great interest that we turn to inspect the works named above, to see how far the two last named books supply the deficiency. Peculiar qualifications are required in the writer of a student's hand-book of any science. The author requires to possess at once a practical acquaintance with the details of his science and thorough knowledge of its bibliography. A writer who possesses but one or the other of these qualities must necessarily fail in being able to instruct thoroughly, correctly, or interestingly. In the compilation of such a manual it is also desirable so far as possible to apportion the space with reference to the relative importance of the different parts of the subject. Of course as far as they go, nothing can be superior to the works of Huxley and Rolleston, on comparative anatomy, but neither of these is sufficiently zoological to take rank as a manual of that science.

We must confess it is with disappointment that we close both the works on systematic zoology named above, and cannot help saying that though Rymer Jones' work has now reached its fourth edition, it is far below the proper standard to which such a book should attain; with all the advantages of good printing and paper, profuse

and well executed illustrations, and the opportunity of benefitting by the criticisms on three former editions, the book does not commend itself as a reliable class-book. Scarcely any science has made such progress within the past fifteen years as comparative anatomy, and yet from this work one would think that zoologists and other students of nature had been sleeping for ten years at least. In the first instance the author makes no reference whatever to the monera, those singularly low organizations which form the lowest round of the ladder of animal life, and there is not even the smallest reference to the paradoxical bathybius, coccoliths, and coccospheres, whose nature have been so recently expounded by Prof. Haeckel.

It is very hard indeed, in reading this book, in any of the parts of the animal kingdom to understand the classification of the author, divisions are throughout so loosely referred to; thus he seems to give up the division into sub-kingdoms as a bad job after the cœlenterata, and it does not at all seem clear as to whether he intends us to consider helminthozoa and echinodermata, as parts of this sub-kingdom, as sub-divisions themselves of the same rank in the predicamental line, or as an anticipatory portion of the articulata, He also regards the cirripedes as a class separate from the crustacea, under which latter he includes the rotifers, and considers the mollusca as an *order* (Index, p. xxiii.) Still farther on he regards the amphibia, even the dipnoi as belonging to the reptilia, and seems also to group the sirenia and the cetacea together. These are a few of the peculiarities in his classification with which we scarcely think many zoologists will agree. The groups of the gregarinidæ he regards as belonging to the helminthozoa, not to the protozoa, and all the details of their structure are discussed in a single short paragraph.

It is not easy to see upon what ground he separates the foraminifera, amœbæ, and actinophrys from the rhizopods, but he seems to regard them all as of separate ordinal value. Not a word is said regarding the affinities of the sponges and cœlenterata pointed out by Haeckel, which, though still a controverted point, yet is sufficiently important to deserve some reference, while in the article infusoria, space is wasted in figures and descriptions of the exploded doctrines of Ehrenberg, and in figures of diatoms and desmids.

The section on cœlenterata is good so far as it is a copy of Greene's Manual, but like that book it is now out of date, and no attempt is made to introduce any more modern observations into this part.

In the section helminthozoa he divides the organism into parasitic forms, and turbellaria, entirely ignoring the host of free nematodes. The first group in this class is that of the gregarines, the second is the cystiform worms, cysticerci and cœnurus, and under these he makes not the slightest reference to the fact that they are all nothing but larval forms, so that a learner would naturally regard them as separate animals, until to his surprise a few pages on he would come to learn that they were nothing but the primary stages of the development of another group of animals.

A similar reticence characterizes his description of the class crinoidea, no reference whatever is made to those most interesting recent forms described by Sars and Wyville Thompson, or to Lovén's genus Hyponome, as far as we know, the only living cystoid echinoderm. As a rule the rough anatomy from echinoderms is not badly given, and the descriptions and figures of the dental apparatus are excellent.

The chapter on the annelida is very imperfect, and so is the chapter on the myriapods. Not the smallest reference is made to that most singular of modern zoological discoveries, the presence of a notochord in the larva of the ascidian mollusca, discovered in 1866 by Kowalevsky, but fore-shadowed by Krohn, and still more recently worked out by Metschnikow and Kupfer. This must be regarded as an exceedingly grave defect, as the embryogenesis of the ascidians (now thoroughly well known) constitutes so important a link between the vertebrates and invertebrates.

In the description of the mollusca which is, on the whole, moderately good, we look in vain for any mention of the structure of shell, the relations of the operculum, or the relations of the shells of gasteropods to those of conchifers.

The section of the vertebrates is the poorest in the entire work, leaving out of account the peculiarities of classification noted above, such as the inclusion of the amphibia among the reptiles, &c., there are many statements made which are scarcely reliable. Thus, few morphologists will agree with him in stating that the scapulocoracoid and clavicle represent respectively the ilium, ischium, and pubis. Much room is also wasted in detailing the relations of the typical vertebra of Professor Owen, and the views of that eminent author, regarding the typical skeleton, are unhesitatingly adopted. That the homologies of the bones of the shoulder-girdle, in the tortoises (p. 727) have not as yet been precisely pointed out, will surprise any one acquainted with the splendid monograph of Mr.

Parker on the shoulder-girdle, and the recognition of the parts of the shoulder-girdle of the crocodile, which he calls the type of the saurian reptiles, is equally curious; certainly the parts of the former are not as Professor Jones suggests scapula, coracoid and clavicle.

While noticing the vertebral column of mammalia, our author refers to the supposed nine cervical vertebræ of the three-toed sloth as a solitary exception, but makes no reference whatever to the still more striking and well-known instance of the manatee, which has only six.

A singular reason is given by Professor Jones for the double occipital condyle in mammalia (p. 799), viz., that it depends on the greatly increased size of the encephalon; this would certainly not account for the presence of two occipital condyles in amphibia (a fact, by the way, not noticed on p. 723 in the description of the skeleton of the frog). These occipital condyles, also in mammalia, Professor Jones describes as forming a part of the basi-occipital bone.

The meagre details of the muscular anatomy of mammals give very little idea of the importance of the subject. Professor Jones regards the diaphragm as a muscle peculiar to mammals, disregarding altogether the fact of its existence in the ostrich. The relation of the palmaris brevis to the panniculus is by no means so clear as our author states. The digastric likewise does not belie its name among the rodentia, a group which includes nearly half the mammalia, so that man and monkeys are not the only animals with a biventral digastric. The description of the large triangular muscle as existing in cetacea, p. 823, is scarcely recognizable by any one familiar with the myology of animals of that group, and apropos of the same order, Professor Jones considers the rudiment of the pelvic girdle in the whales to consist of the pubis, while our highest authorities on the subject (Flower, p. 291) regard it as the ischium. The fact that Eschricht, Reinhardt, Lilljeborg, and Flower have found not only an ischium, but a rudimental femur, is not only unnoticed, but the non-existence of any other part of the lower limb is predicated. The old exploded theory of Cuvier regarding the spouting of the whale, though so plainly contrary to anatomical fact, yet is given as if thoroughly ascertained, and the absence of nerves of smell, and olfactory lobes, though asserted by Professor Jones, cannot be admitted as true in globiocephalus or balænoptera.

Many more statements in this book strike us as being equally deserving of animadversion, but fault-finding is an unpleasant and

thankless task, and we hope for the credit of the author that more care will be taken in future editions to bring the matter of the book up to the present standard of science.

We turn to the less pretentious and more portable volume of Dr. Nicholson's, and though it is free from many of the grave faults just noticed in Rymer Jones' Manual, yet it has defects of its own which strike the eye and the mind of a reader painfully. If the statements contained in this work are true, it is because they are copied to a large extent by the scissors from different accessible English works, and the inverted commas are scattered through the book, as if dusted out of a pepper-caster; in twelve pages taken at random, they averaged nine pair on each page; in fact, if we take away the paragraphs, either quoted or adapted from Huxley's Introduction, Owen's Comparative Anatomy, Woodward's Mollusca, and Greene's Manuals, we would reduce the contents of the book to an exceedingly small size. Now, of course, it is not expected that the author of a manual has much scope for originality; yet, we cannot but think that one has no business to write a book unless they have got the capacity to weave into continuity the necessary ideas selected from various sources. The only quotations which in such a case are pardonable are those from original memoirs, not those from class books, any one of which is equally accessible to any student. Now, we do not mean to say that Dr. Nicholson may not be an exceedingly able zoologist, but when an author will not trust himself in the expression of a zoological definition or description, and habitually borrows these, it certainly lays him open to the suspicion of not being sufficiently conversant with the subject, to justify him in assuming to instruct others.

Dr. Nicholson also has fallen into some of those weaknesses which are inseparable from authors who quote others at second and third hand, and his evident unfamiliarity with the modern Continental zoological authors renders the work not quite up to the work of a first-class hand-book. This is very palpable on his article on the Radiolaria. The other work by this author, the Introductory Text-book is, we think, far better than this, being more modest in its pretensions, and far more suitable for the advanced schoolboy than the Manual is even for the most junior college student. The good, English, Student's Manual of Zoology has yet to be written.

It is a pleasure to turn from these works to the little volume by

Professor Flower; one of the most solid and thoroughly valuable contributions to comparative anatomy which has ever yet been given to the scientific world. The book is portable; the illustrations clear and perfectly satisfactory, and everything stated in the book as fact we believe to be perfectly reliable. The language is clear, concise, and suitable, and though the work is necessarily one of detail, yet it is surprising how thoroughly Professor Flower has succeeded in making it interesting. The points in this book interesting to the human anatomist are very many, and as a book of reference on comparative osteology, this must form part of the library of every anatomist. Professor Flower indicates (p. 60) the distinction among the so-called sacral vertebræ of man between those which have an appended costal element, and those without, which he calls pseudo-sacral. He also (p. 134) calls attention to the hitherto unnoticed tympano-hyal segment in the human skull, a cylindrical piece of bone, truncated below about $\frac{1}{20}''$ in diameter, seated in a depression in the hinder border of the tympanic bone immediately to the anterior and inner side of the stylo-mastoid foramen.

The last chapter in the book is devoted to the correspondence between the anterior and posterior extremities, and Professor Flower deserves great credit and thanks from all morphologists for the line of thought which he has wrought out in this chapter. The only theoretical consideration in this system of comparisons, that we think Professor Flower has not perfectly established, is that of the rotations of the components of the shoulder and pelvic girdle. The various points referred to in relation to the morphological relations of the femur, humerus, and other bones of the extremity, commend themselves to the approval of any comparative anatomist. The parts of the shoulder and pelvic girdle are mainly twofold; the scapular or iliac, and the coracoid or ischio-pubis; the former of these seems to be a modification of a prismatic bone capped at its neural end by the superscapula; the three surfaces of the scapula are prescapula, postscapula, and subscapular fossa; the three margins are preaxial, or the cervical costa, mesaxial or the spine, and post-axial or the axillary costa; of the ilium the surfaces corresponding seem to be preiliac or iliac fossa, postiliac or dorsum, as far as the middle arcuated line, and subiliac or that part of the dorsum ilii under the line; the three margins then being, iliopectineal, crest and middle curved line. The correspondences suggested by Professor Flower differ from these in some few respects, as will be seen by the following table:—

Ideal	Scapular	Mr. Flower's Theory	Ilium (R.)	Ilium (Flower)
Preaxial surface	Prescapular fossa	Postscapular -	Iliac fossa -	Iliac fossa
Mesaxial -	Subscapular -	Prescapular fossa	Dorsum ilii below linea arcuata media	Facies auricularis and posterior part of inner surface
Postaxial -	Postscapular -	Subscapular fossa	Dorsum ilii above line	Gluteal surface
Preaxial edge -	Cervical costa -	Spine - -	Iliopectineal line	Linea arcuata interna
Mesaxial edge -	Spine - -	Glenoid border -	Iliac crest -	Ischial border
Postaxial edge -	Axillary costa -	Coracoid edge -	Linea arcuata media	Acetabular border

The difference between these two theories depends on the fact that Mr. Flower regards the prismatic bone as having undergone a rotation on its own axis, while according to the other theory the two girdles are supposed to have undergone no alteration in position around this axis, and only a slight rotation on the horizontal transverse axis, which has been in a different direction in the two extremities. In the lower the ilium has moved forwards on the acetabular axis while the ischiopubis has moved back, and in the shoulder-girdle the scapula has moved slightly backwards, while the the coracoid has been as slightly advanced. We hope this work on the osteology of the mammalia is but the forerunner of a series from the same pen on the osteology of other departments of the vertebrata, illustrated from the splendid treasures of the Hunterian Museum.

On Comparative Longevity in Man and the Lower Animals. By E. Ray Lankester, B.A.; Junior Student of Christ's Church, Oxford. London: Macmillan and Co. 1870. Pp. 135.

This work is a prize essay on a popular but puzzling subject, and contains a considerable amount of information, gathered from numerous sources. It is an attempt to reduce to a scientific form the numerous desultory observations which have been made from time to time on the duration of life in individuals. Longevity is regarded by the author as the length of time during which the phenomenon of life is exhibited in the individual, and it may be regarded as average, or as potential longevity, the former being the average period of life in a number of the individuals of one species, allowing for the effects of accident and disease, the latter being the period during which the organism is ordinarily capable of

retaining vitality when uninfluenced by either of these disturbing causes. The potential longevity of different species is subject to variation, and Mr. Lankester endeavours to analyse the agencies which are at work in causing this variation. The growth of the organism progresses as long as protoplasm is formed more rapidly than it is specialized. As growth progresses expenditure increases, until the waste equals the supply, and as long as this condition of equilibium can be maintained so long will life continue; hence some reptiles, trees and fish, exist through enormous periods of time. But synchronous with the cessation of growth, reproduction sets in, a new source of waste which more or less rapidly exhausts the potential energy of the individual, and brings on its dissolution. Thus to some extent according to the time of the development of the reproductive functions, is the potential longevity of the individual. The main feature in the book is that it is an attempt to apply the evolution system of philosophy to the explanation of the varieties of longevity. The author is under considerable disadvantage in the small number of data on which he has founded his reasoning, but has produced a very creditable volume notwithstanding.

Practical Lithotomy and Lithotrity, or an Inquiry into the best Modes of removing Stone from the Bladder. By Sir Henry Thompson. Second Edition, considerably enlarged. London: J. & A. Churchill, New Burlington-street. 1871. Pp. 327.

A Paper on Median Lithotomy. By James L. Little, M.D., of New York, Surgeon to St. Luke's Hospital—from Transactions of the American Medical Association. Philadelphia: Collins, 705, Jayne-street. 1870.

In the new edition of his work on *Lithotomy* and *Lithotrity*, Sir Henry Thompson has added much new matter, and removed some of what the first edition contained. Three entirely new chapters are added; one of these is on the results of lithotomy; it embraces an analysis of 1,827 cases, and places fairly before the reader the general result of lithotomy, previous to the introduction of lithotrity. Another of the newly written chapters is on the important topic of the employment of lithotrity in cases where, besides calculus, serious organic disease, or other complication exists. It deals, in fact, with the complication of stone, such as stricture, enlarged prostate, paralysis of the bladder, tumours of the bladder, renal

disease, &c., and forms a most important addition to the original work. Another chapter gives the general results of lithotrity, drawn from the author's own experience, as well as that of Civiale, Brodie, and others, while an appendix contains brief details of 204 cases occurring in Sir H. Thompson's own practice. The work is much more than new editions generally are; it is in many important respects a new book, and will add to the author's already well-earned reputation.

Dr. Little's brochure on *Median Lithotomy* contains only twenty pages, but in these are recorded the experience of twelve cases, and a tabular statement of forty cases in which this operation has been performed by American surgeons. Dr. Little claims for the median operation considerable advantages over the lateral or bi-lateral procedure—1st, He says there is greater facility in entering the bladder; 2ndly, less liability to hæmorrhage; 3rdly, less danger of wounding the rectum; 4thly, no danger of wounding the deep perinæal fascia, and, consequently, of infiltration of urine; and 5thly, he says the patient retains control over his bladder after the operation. If the experience of other surgeons is found to justify this last conclusion, the benefit arising from it alone would do much to establish the median operation, as one deserving of being more frequently performed than it is in this country. Dr. Little's paper is illustrated with several well executed woodcuts, which make his explanations very lucid.

WORKS ON MALARIA AND INTERMITTENT FEVER.

1. *What is Malaria?* By C. F. Oldham, M.R.C.P.E., M.R.C.S.L., Assistant Surgeon, H. M. Indian Forces. London: H. K. Lewis. 1871. 8vo, 186 pages.

2. *Traité des Fièvres Intermittentes.* Par Leon Colin, Médecin Principal de l'Armée. Paris: Baillière et Fils. 1870. 8vo, 533 pages.

What is malaria? This question, the universal interest of which cannot be doubted, has from the earliest ages of medicine afforded a fruitful field for argument to medical writers; yet even at the present day no uniformity of opinion is found to prevail among professional men on the subject. To answer this question Mr. Oldham now addresses himself, having made careful investigations,

not only by studying the writings of others, but, which is of much greater value, by studying the disease itself.

Having pointed out how important to persons inhabiting all parts of the globe is the determination of this question, our author proceeds to examine the various theories which have been put forward on the subject, pointing out in each case the objections which have, from time to time, thrown doubt on them, or caused them to be altogether condemned as untenable. A large space is occupied in showing how unsatisfactory is that doctrine which has of late years been so generally adopted by text books on the subject, namely, that malaria owes its origin to exhalations from decaying organic matter in water or soil. It has, indeed, long ere this struck those who held this doctrine that anomalous cases were of not infrequent occurrence, in which persons were affected with intermittent fever in localities where no such influences as marsh miasm or decaying organic matter were to be found, and that, on the other hand, certain districts, where all the conditions supposed to be necessary for the propagation of ague were present, enjoyed a remarkable immunity from the disease. Mr. Oldham states, that having begun life with a firm belief in "malaria," it required years of careful observation in infected districts to disabuse his mind as to the truthfulness of his faith.

The two following paragraphs, in which the author states to what conclusion his own observations have led him, may be looked on as presenting the key to the entire work. He says—

"Careful observation convinced me not only that malaria, as a specific poison, does not exist, but that the cause of the diseases attributed to it is chill, or, in other words, the sudden abstraction of animal heat."

And again—

"Damp cold is only more frequently connected with malaria than cold which is dry, because of its greater power for the abstraction of animal heat; for which the capacity of the vapour of water is equal to 16,000 times that of dry air."

According to Mr. Oldham, then, there is required for the production of ague and its allied complaints no marshy ground, rank vegetation, or any of the conditions hitherto looked on as necessary, but simply a rapid change of temperature, which, as he points out, is most likely to produce a sensible effect on the body when the air is loaded with moisture. In this way he explains the apparent

anomaly that the disease has been found to rage on arid plains or barren rocks with an intensity almost equal to that found in the densest jungle of India.

Mr. Oldham's work shows much and careful research, and, containing much information in a small space, will well repay a perusal; but we fear that the theory advocated by him will not be found adequate to explain all the phenomena exhibited by malarious diseases. Of these we may mention one especially which he has neglected to explain, namely, that certain places which were formerly thickly populated, and nearly or altogether free from this class of ailments, are now rendered utterly uninhabitable by their prevalence. In putting forward any hypothesis it is evident that such well-established facts as these must be taken into account, or the theory will obviously fall short of the truth.

On turning to M. Leon Colin's book we find opinions of a totally different nature advocated. He agrees with Mr. Oldham in rejecting altogether the idea that the presence of marsh miasm is in any way necessary for the production of ague or its allied diseases; but here all agreement between the two authors may be said to end. In the following words M. Colin gives us his theory of the origin of intermittent fever:—

"Pour moi, la fiévre est causée avant tout par la puissance vegétative du sol quand cette puissance n'est pas mise en action, quand elle n'est pas épinsée par une quantité de plantes suffisante pour l'absorber."

Six years spent in Algeria, and with the French army of occupation at Rome, have given our author the power of speaking with considerable authority on the subject of malaria; at the latter place especially his attention having been almost entirely devoted to observations on malarious diseases. The first part of the work, on the etiology of the disease, is founded altogether on investigations made at Rome, and our author enters fully into the conditions which, in his opinion, contribute to the development of malaria as found in that city. These conditions he states to be three in number.

1. The configuration and constitution of the soil (conditions telluriques).

2. Atmospheric influences (conditions météorologiques).

3. The resistance offered to the disease by the collection of human beings in masses, as in a town (conditions sociales).

This part of the work contains much of interest, not only with

respect to the actual etiology of the disease, but also with respect to the history of its origin and progress in and around Rome, as far back as any reliable information can be obtained on the subject.

In the second division of the work our author treats of the symptomatology of the disease, and for convenience describes it under two heads, the acute and chronic. He insists especially on the fact, that it is an error to suppose that periodicity is by any means a fundamental character of this class of affections, but holds that the fever can assume either of three forms, the intermittent, remittent, or continuous, and that the form or type under which it shows itself is determined—1st, by the degree of temperature, and 2nd, by the date of the primary infection of the system. This opinion has, indeed, been previously put forward by M. Boudin, and, if generally adopted, would obviously alter considerably the generally received ideas of the disease, of which the periodicity has by most authors been looked on as its chief characteristic. Of this division of the book a large part is devoted to the description of those forms of the disease to which M. Colin gives the name of "fiévres pernicieuses," by which term he indicates those cases in which the ordinary course is interrupted, or the symptoms replaced, by dangerous complications, and of these he describes a great number of forms.

To the chronic form of the disease, which is commonly known as paludal cachexia, but a small space is afforded. We must, nevertheless, pause here to mention the result of his observations on the blood of cachectic patients. In it he has found alterations to have occurred corresponding to the cutaneous and splenetic pigmentation; and from this he argues that the disease is not a simple condition of anæmia resulting from general unhealthy conditions, as often urged; but that the blood presents peculiar characters, some apparent to the naked eye, and others only to be discovered by microscopic examination.

Two chapters here follow on the diagnosis and prognosis of the disease, and we are then brought to the interesting subject of the pathological anatomy. Here we have an account of the organs usually found affected—the spleen, liver, lungs, heart, &c., attention being more particularly directed to the marked difference found in the lesions in acute and in chronic cases.

The treatment and prophylactic measures to be adopted, occupying the remaining chapters of the book, are discussed at considerable length; the use of quinine and its substitutes, and the

effects of antiphlogistic treatment, being the points especially dwelt on.

M. Colin's work is indeed worthy of perusal by all members of the profession interested in the subject of which it treats. The chapters on the etiology and the pathological anatomy of the disease are especially worthy of attention, the latter more particularly being extremely clear and precise without being unnecessarily prolix. Whatever may be the ultimate fate of the theory which our author advocates as to the etiology of the disease, it may be said to have, at least, the advantage of explaining the known facts connected with ague in a manner altogether more satisfactory than has been done by any theory hitherto advanced. Moreover, his accurate investigation of the varied manifestations of the disease, and his clear exposition of his own views regarding it, will render the work of value to all medical men.

The Dublin Practice of Midwifery. By H. Maunsell, M.D., &c., New Edition, enlarged and revised. Edited by Thomas More Madden, M.R.I.A.; Senior Assistant Physician, Rotunda Dublin Lying-in Hospital, &c. London: Longmans. 1871. Fcp. 8vo. Pp. 315.

The preface to the first edition of this work is dated August, 1834. It is therefore 36 years since Dr. Maunsell first issued it to the world. At that time the author was a teacher of midwifery, and the work was not only admirably suited as a hand-book to be used by the pupils attending his instructions, but fully accomplished the author's object, which was, as he states, to give a concise, but, at the same time, a sufficiently full and perfect account of practical midwifery, and also to indicate the many points of practice in reference to which the lessons of the Dublin School differ materially from those inculcated in the works of such distinguished authors as Denman and Burns, Ramsbotham and Merriman. Accordingly the book was eminently successful, and a favourite in the Dublin and London Schools for many years.

The Dublin practice of midwifery, however, of 1834 is not the Dublin Practice of 1871; obstetric art and science have made great progress throughout the world, nor has Dublin been behind hand. It would be little better than a libel and insult to set forth the teaching of 1834 as that of 1871. The publishers have, therefore,

thought it well in bringing out a new edition, to have it revised and edited by a physician of the present day. The task of preparing this has been committed to Dr. Thomas M. Madden, Senior Assistant Physician to the Rotunda Lying-in Hospital, and it is with extreme regret that we feel coerced to record our protest in the strongest terms against the work thus edited being taken as an exposition of the Dublin Practice of Midwifery of 1871. With both our hands we protest the Dublin School of Midwifery has not been asleep; it keeps progress with its fellows, and if not in advance of them is not behind.

It is with regret that we have to write this, and to justify our doing so, we must refer to the alterations and additions that have been made in this edition, which are stated by the editor in his preface, as follows:—

"The twelfth chapter on the Use of the Forceps has been completely re-written. The fourteenth chapter on the Treatment of Hæmorrhage has been considerably enlarged and altered. An additional chapter on the present Type and Treatment of Puerperal Fever has been appended. The following chapters—'on the Third Order of Difficult Labours,' 'on Version in Cases of Pelvic Deformity,' 'on the Cæsarian Section,' 'on the Induction of Premature Labour,' and that 'on Inversion of the Uterus,'—are all entirely new."

With Dr. Madden's statement that "there has been hardly any improvement in the modern practice of medicine or surgery greater than that which has recently taken place in the practice of midwifery from the more frequent and timely employment of the forceps in those cases which require its application," we most fully and cordially agree, but in his recommendation of a forceps measuring "10 inches in length from handle to joint, the blades $6\frac{1}{2}$ inches long, the widest space between blades when locked $2\frac{7}{8}$ inches, the space between the points $1\frac{1}{4}$ inches," we cannot concur.

In speaking of the different classes of cases that require the forceps, the editor has quite overlooked that class of cases so often met with, and in which that instrument is so peculiarly useful. We allude to those in which the head is coming in the first or second position, but from some disproportion between it and the pelvis it is arrested in its course, after a certain portion of it has passed through the brim, and long before it has reached the perineum. These are the cases that in former years were sacrificed by the deadly perforator, but are now safely delivered by the aid of properly

constructed instruments. The omission of such an important class of cases, and the method of dealing with them, leaves this chapter re-written as the editor tells us by himself in a very lame and imperfect condition. It cannot be from non-acquaintance with such cases, for Dr. Madden knows that they are the frequent cause of the employment of the forceps in the Rotunda Hospital, where he is still assistant, and that the instrument in use in that institution for years is that of Dr. Beatty, measuring 12½ inches in length.

In describing the midwifery forceps, the editor says, "There are two principal varieties of forceps, namely, the short and the long forceps;" and then he goes on to describe and recommend a poor imitation of the long-ago exploded and forgotten very short forceps of Smellie, and no other. It would seem as if he had been asleep during the last thirty years, and suddenly awoke, armed with that old and very inefficient instrument, and had never heard of the vast improvements that have taken place in the interval, and the good reasons for which these changes were effected. We confess we were startled at finding in a book, professing to be the exponent of the Dublin practice of midwifery, the improved elongated forceps of Simpson and Barnes, of Murphy and Graily Hewitt, and of Beatty and Churchill, almost totally ignored, and the dwarfed feeble instrument already spoken of, set before students in midwifery, as the one to be used.

Smellie's forceps did well enough when no one thought of using the instrument until the head of the child was resting on the perineum for some hours; but when in the advance of science cases in a far earlier stage of labour came into the domain of the forceps, and were totally out of reach of the short instrument then in use, longer ones were introduced which would deliver heads as well those on the perineum, as those much higher up in the pelvis, and have been universally employed until just now, when we regret to learn the senior assistant of the Rotunda Hospital has retrograded to resume the use of an obsolete implement. We trust the present distinguished master of that great institution has not allowed the pupils attending there to remain under the delusion that that is "Dublin Midwifery." We have before us, as we write, an instrument made in this city, thirty years ago, closely resembling that now described by Dr. More Madden, but half an inch longer, which was cast aside by its inventor, when he found it incapable of reaching a head, one-third of which had entered the brim of the pelvis, and was there impacted.

If Dr. Madden chooses to amuse himself with his toy forceps no one could object, but he has no right to disfigure a book of such well-deserved reputation as that of Doctor Maunsell, by introducing such matter into the text, and still less right to lead unwary students into the belief that they could accomplish all that the forceps is now called upon, and expected to do, with such means.

The chapter on the treatment of hæmorrhage, we are told, has been considerably enlarged and altered. The chief and almost only alterations we find in it consist in the addition of three or four paragraphs, and an alteration in another. The recommendation to administer a full dose of ergot as soon as the head begins to press on the perineum, in cases where we have special reason to expect *post-partum* hæmorrhage, "and in every case where a woman has previously borne a number of children," is the substance of the first of these paragraphs, and was long ago advised by Dr. Beatty.

In the next paragraph we are directed, when other means fail, "to inject cold water, by the vaginal syringe, into the cavity of the uterus, as long as the water injected returns tinged with blood," and in the third we are told in very brief terms that if this fails we must try Barnes' method of injecting perchloride of iron, but we fear that if we adhere strictly to the first directions there will be but few patients left on whom to try the styptic effects of the iron.

In another paragraph Dr. Madden states more clearly than had been done in the former edition, the uses of opium in hæmorrhage, and then he gives a brief account of Dr. R. M'Donnell's method of performing transfusion, with which the chapter closes. There is no change in the teaching of the old editions, as to the impropriety of turning and effecting delivery in cases of accidental hæmorrhage, nor any allusion to the methods of treating placenta previa, advocated by Radford and Simpson, or to that of Barnes, or to that taught by some of our American *confreres*, without some account of all of which a statement of the practice of midwifery in Dublin or elsewhere is not complete.

In the chapter on the third order of difficult labours, a general protest is entered against craniotomy, even when the child's head is hydrocephalic to such an extent that it cannot pass through a normal pelvis, or in cases of great flooding before delivery where the forceps cannot be applied, or in similar cases of convulsion, rupture of the uterus, and other varieties of complex labour; and again we protest that neither in this nor in his advocacy of the Cæsarian section does the editor fairly represent the Dublin School

of Midwifery; nor is it a fair representation of the Dublin School to imply that its professors are so ill-informed as not to understand the difference between the *intra uterine* douche of Cohen and the *vaginal* douche of Kiwish for inducing premature labour, and we rather think no one will be more surprised than the present learned Professor of Midwifery in the University of Dublin himself, to learn that he has invented an instrument for the induction of labour by means of the *intra uterine* douche.

We hasten, however, to the conclusion of our task, which we assure Dr. Madden has been a most unpleasant one, but the duty has been laid on us, and we think we have done enough without going into the further alterations made in this edition to justify our assertion that it is not a fair exposition of "The Dublin Practice of Midwifery."

The Treatment of Surgical Inflammations by a new Method, which greatly shortens their Duration. By FURNEAUX JORDAN, F.R.C.S., Eng.; Surgeon to the Queen's Hospital, &c.

"IN all disease, in life, in death, in science, we only know phenomena; so in inflammation. We know nothing except its phenomena—phenomena pathological and clinical." The work before us from which we quote this passage is itself full of phenomena. It consists of 158 pages of letter-press, 27 of illustrations, and 45 of packing. Surely the phenomena of inflammation, pathological and clinical, are not such as the author represents in his illustrations. He seems to have been half conscious of this when he wrote the preface to them:—"The following diagrams are mostly drawn from nature" It would, indeed, be hard to find in nature a specimen of whitlow of the thumb, or a palmar abscess, such as his diagrams represent, and still more difficult, we believe, to find a specimen of either disease which would be disposed to yield to the author's treatment. The author clearly lays claim to the possession of a logical mind, for before entering on a detailed examination of the best remedies for inflammation, he says—"To logical minds, no remedy is the worse if it is capable of intelligent explanation." Yet we find, we think, the principle which has guided his mind throughout the examination, expressed in the following passage:—"Having discovered the best remedies in some inflammations, why not use them in all?" It is not necessary to go through the author's reasonings; we give his summary. "The remedies then, which appeared most

reliable in inflammatory diseases were counter-irritation, pressure, rest, elevation, and removal of cause. My rule of treatment was this in every inflammation (discarding for the time authorities, books, teachers and friends), how and where can I best employ counter-irritation? How best, and to what degree can I use pressure? How can I best secure rest? Can I elevate the part? Have I taken every care to remove the cause?" "Internal treatment did not appear unimportant; on the contrary, in specific inflammations I firmly believed as the result of daily observation, in the great utility of mercury, and iodide of potassium. In all inflammations I gave iron. Often, as the great rester, I found opium invaluable." How the adoption of any or all these modes of treatment in suitable cases can be called "a new method" we fail to see, any more than we can see the necessity for the author abandoning all authority, and even cutting his friends when called to treat a case of inflammation. The author believes "that incisions are often an excellent form of counter-irritation, and that it is in this way that they are beneficial in the majority of abscesses." "In a small minority where abscesses are under dense and tense tissue, they have other and obvious benefits." The author suggests that such disease as thecal paronychia, for instance, in the most aggravated form, should be treated by painting the limb with tincture of iodine. He says, "with the treatment for whitlow, just described, incisions are often unnecessary. The objects of incisions are two—to relieve pain and to save deeper structures, bone, or joint, or tendon; both these objects are gained more rapidly and more pleasantly by a quick acting counter-irritation." "In a few exceptional cases, where subsidence of pain and swelling are not immediate, an incision should be made without delay." We fear that the author's experience of "three or four years" in the treatment of inflammation according to this plan, has taught him more of the art of book-making than of good surgery.

A Manual of Medical Jurisprudence for India, including the Outline of a History of Crime against the Person in India. By NORMAN CHEVERS, M.D.; Surgeon-Major H. M. Bengal Army; Principal of the Calcutta Medical College; Professor of Medicine, &c., &c. Calcutta: 1870. Pp. xxx. and 862. Royal 8vo.

ALTHOUGH the principles by which medicine is guided, as an auxiliary to law, must be the same everywhere, yet the occasions

for its application will be diversified and variously influenced by local conditions of society, of morals, and of civil government. A medical jurisprudence for India is consequently not to be regarded as a superfluous addition to the continually increasing literature of this important subject, but rather as a much needed contribution to that store of knowledge which all concerned in the administration of justice throughout British India ought to have readily available for use. In the peculiar circumstances of our East Indian possessions, the result of an antique, enduring, but unimproving civilization, and of a state of morals pervaded and perverted by a sanguinary polytheism, rendered perhaps more intense by a political or national hostility to the religions of the various conquerors who have successfully overrun Hindostan, it is not surprising that a devotion to the destructive power or principle of evil should extensively prevail; and this, by the institution of caste, is still further aided, so as to become hereditary and perpetual. To aid the medical practitioner, called perhaps unexpectedly to elucidate the circumstances of some atrocious crime springing from such a social system, there were only such works as those of Taylor, Guy, Christison, or Flandin, all of considerable, indeed of great merit as scientific authorities, but deficient, necessarily deficient, in what the European in the East so much requires, that is, adaptibility to the special circumstances of the country and its various populations.

In 1854 Dr. Chevers published his *Report on Medical Jurisprudence in the Bengal Presidency* (*Indian Annals of Medical Science*, October, 1854), which was so highly approved by the Government of India, that the author was induced to enlarge it, so as to form a distinct volume, which, under the title of *A Manual of Medical Jurisprudence for Bengal and the North-Western Provinces*, was published at Calcutta in 1856, at the expense of the public revenue, four hundred copies being taken to be distributed among the judicial officers. The present work Dr. Chevers designates a third edition, but it is so far re-written, as well as augmented, that it rather deserves to be regarded as a new work, which, except in its subject, has very little in common with its predecessors, which bear the same relation to it that the early sketches of the artist do to his finished picture.

The peculiarities of race, of caste, and of religion among the natives of India, present many objects for observation in the investigation of criminal charges. The character of the weapons

ordinarily used in the various provinces, and the powerful influence of traditionary customs in prompting to deeds of violence, have each to be taken into account by the medical and legal officials to whom they may be referred. In these inquiries the medical officer is not only guided by his own skill and observation, but also by the amount of information whatever that may be, which has been already obtained by the police regarding the subject of the inquiry. It is obvious, that although this provision of the Anglo-Indian code of criminal procedure is intended to assist the medical investigator, it might, in circumstances that are readily conceivable, become a source of error, at least, we trust that the medical officer, while he receives such reports, will not rely on them so far as to contradict his own induction from what he actually observes while examining the human remains, or other objects submitted to his inspection.

The atrocious and insidious murders committed by the *Thugs* appear to be still of sufficiently frequent occurrence, in India, to require the attention of the medical jurist. The more usual mode of effecting this crime by strangulation or suffocation, appears to have been repressed by a rigorous execution of the law against it; but in the use of poison the Thug finds an easier mode of destroying his victims, and at same time one which is more difficult for justice to trace to the perpetrator. This system of poisoning is common throughout the three Presidencies, and "is pursued systematically as a trade, not being merely the device of a stray criminal here and there" (p. 158).

Venomous serpents contribute largely to the mortality of some Indian districts. A Government report is quoted (p. 369), according to which the ascertained number of deaths from snake-bite during the nine years 1860-1868, in a population of five millions seven hundred thousand, amounted to more than nine thousand two hundred; that is, if the police returns are to be relied on as credible evidence.

Among the more curious cases mentioned is one of sudden death, occurring in the first access of an attack of tetanus, described by the late Dr. Allan Webb, of Calcutta, who had the medical charge of the *Martiniere* School, where the case happened: "An apparently healthy boy, one of the pupils, was seated on his bed, having a small sore on his foot dressed by the native doctor. Having applied the dressing, the native doctor was leaving. He walked straight to the door; but as he was passing out he heard a noise from the bed. Turning, he saw the boy supported on his

occiput and heels, his body being arched up in opisthotonic spasm. He ran to the bed, the body sank, and death was immediate" (p. 249). To complete this case, there should have been made a careful chemical examination of the dressings, and of the ulcerated surface, and also of the intestinal canal, and the last matters swallowed by the deceased previous to the tetanic paroxysm. Dr. Chevers adds:—"The first case of tetanus which I attended in India proved fatal in less than twelve hours" (*Ib.*)

Alcoholic poisoning is described as a frequent cause of death, or a cause to which death is attributed; but Dr. Chevers qualifies the statement by saying:—"It is probable that many who are supposed to die from alcoholic poisoning are, in reality, choked by vomited food lodged in the bronchial tubes" (p. 250). He adduces no cases in support of this conjecture. In connexion with this subject he mentions a practice that might with advantage be adopted in other great cities:—"In Calcutta, it is usual to send men who are found dead drunk in the streets to the hospital; and I am sure that many lives are thus saved during the eight hot months of the year, in which these unfortunates' danger is doubled by a tendency to insolation." . . . . "I have noticed that while, generally speaking, *delirium tremens* is very amenable to treatment in moderately cool weather, those cases which occur in 'sunstroke weather,' especially between the beginning of May and the 20th of June, are mostly fatal" (p. 250). Drunkenness would be checked, even where it is most prevalent, if it were never admitted as an apology for crime. It should be regarded not as an extenuating, but as an aggravating circumstance; and should never confer exemption from responsibility, nor immunity from punishment. We read therefore with pleasure (p. 805) that "in cases of homicide and wounding, a plea of intoxication is not admitted by the Mahomedan law to bar exemplary punishment." This is an example that well deserves the attention of all who would check drunkenness, and its evil consequences in this country.

Those who reflect on the continuous efforts that now for nearly half a century, have been made to give to India the true civilization of Christianity, must grieve when they learn the little success that has been obtained. Infanticide still prevails, and is regarded not as a crime but as a custom. Human sacrifices are still practised. The funeral piles, on which the living and the dead are consumed together, are yet kindled, and all the sanguinary rites of Siva and Kali are still practised, with little, if any, check from law

or public morals. The destruction of female infants is practised systematically; and in some provinces to such an extent as to reduce the number of the female population far below that of the male, thus inverting the natural proportion of the sexes, where this practice, whether it be one of national manners, or of the Brahminical religion, is known to prevail (p. 750).

Dr. Chevers had been already for many years advantageously known by his researches on prophylactic medicine, and on cardiac diseases, as well as by his previous works on the *Medical Jurisprudence of India*. In the present volume, which embodies the results of his extensive and varied learning, and his matured experience, the medical practitioner has a readily available treasure of information on the various topics of medico-legal inquiry that are likely to occur in any part of British India, or of its tributary and dependent states. But it is not only to the medical officer in India that Dr. Chevers' work is important; it is equally, if not more, necessary to all who are there engaged in the administration of justice. In no other book can either medical or judicial officer find collected together such an amount of authentic information on the varied topics of Indian crimes, and the medico-judicial investigations that they require. The Government of India acted with true wisdom when it caused the former edition, if so it may be called, of this work to be distributed among its judicial officers. In its present enlarged and much improved form, the work is more deserving of such patronage, and must be regarded as indispensable to the medical practitioner, to the lawyer, the magistrate, and the judicial authorities throughout India. We know few professional books of which we would be so well justified in saying to our readers of the Indian service—*Nocturna versate manu, versate diurna.*

On Aphasia or Loss of Speech, and the Localization of the Faculty of Articulate Language. By Frederic Bateman, M.D., M.R.C.P.; Physician to the Norfolk and Norwich Hospital. London: John Churchill and Sons. 1870.

Dr. Bateman has published, in a handsome volume, a series of papers, contributed by him to the *Journal of Mental Science*. In this work every contribution of any importance on the subject of aphasia is discussed with care, and the whole subject investigated with much patience and ability.

From his study of the recorded cases of aphasia, Dr. Bateman comes to the conclusion that none of the theories of the localization of speech can be regarded as well founded, inasmuch as while instances which seem to support each of them can be adduced, yet there are to be met with so many cases which go to disprove them, that it is impossible to avoid the conclusion that none of them is satisfactory.

When he comes to offer himself some general remarks on the subject of the faculty of speech, Dr. Bateman offers the somewhat disheartening suggestion that perhaps speech may be "one of those attributes the comprehension of which is beyond the limits of our finite mind."

The impression produced by Dr. Bateman's book is, on the whole, that he under-estimates the importance of the broad clinical facts, that aphasia is, in a very large number of cases, associated with right hemiplegia and with lesion of the left anterior lobe of the brain. Clinical evidence in favour of the frequency of this coincidence is, we think, accumulating rapidly, and in any satisfactory solution of the problem involved in the causation of aphasia this circumstance must be recognized and explained. It is hardly doing justice to this pregnant fact to state, in the words employed by the author (p. 178), that "something may be said in favour of each of the popular theories of the localization of speech." Dr. Bateman has, we think, rightly dwelt on the importance of recognizing functional derangement of the brain, unaccompanied by palpable anatomical lesions. The histology and chemistry of the brain are still by much too imperfect to enable us to pronounce with certainty on the healthy or morbid condition of the organ.

Dr. Bateman's work will be found to present a valuable summary of many of the most interesting cases in which aphasia has been noted, as well as a number of very carefully and fully recorded cases observed by the author, and must undoubtedly be regarded as a contribution of sterling value to the literature of this subject.

A Physician's Problems. By Charles Elam, M.D., M.R.C.P. London: Macmillan. 1869.

Medicine, Disease, and Death, being an Inquiry into the Progress of Medicine as a Practical Art. By the same Author. London: Churchill. 1870.

In the former of these works Dr. Elam discusses a number of

questions of high speculative interest. Although connected together, each essay is complete in itself. The subjects are Natural Heritage, Degenerations in Man, Moral and Criminal Epidemics, Body and Mind, Illusions and Hallucinations, Somnambulism, Reverie and Abstraction. In some of these essays, especially the first three, the author treads on very debateable ground, and propounds views which are not likely to meet with unconditional or general acceptance; but the subjects are discussed in an earnest and philosophic spirit, and the opinions of the author are deserving of attentive consideration. The essays will be found to contain much curious information on various interesting points, and will well repay perusal.

The second work of Dr. Elam consists of some papers published by him in the *Lancet*, with the strictures of that journal upon them, and Dr. Elam's rejoinder, together with criticisms on some recent medical publications.

WORKS, ON PUBLIC HEALTH AND SANITARY SCIENCE, &c.

1. *Second Annual Report of the State Board of Health of Massachusetts.* Boston: 1871. 433 pp.

2. *On Some Advantages of Animal Vaccination for the Prevention of Small-pox.* By A. Vintras, M.D. London: J. & A. Churchill. 1871. 21 pp.

3. *Letters on Vaccination.* By W. Woodward, M.D. Worcester: 1870. 30 pp.

4. *Small-pox and its Prevention.* By E. Crisp, M.D., &c. London: R. Hardwicke. 1871. 24 pp.

5. *De la Variole et de Vaccine.* Par le Dr. E. Chairou. Paris: J. B. Baillière et Fils. 1870. 63 pp.

6. *Investigations in the Military and Anthropological Statistics of American Soldiers.* By Benjamin A. Gould, Ph. D., &c. New York: Hand & Houghton. 1869. 655 pp.

The large volume issued by the Massachusetts Board of Health contains a great amount of information upon sanitary matters, of

which a large proportion is original. We hope to see many similar volumes published by others of the States.

The pamphlets on Small-pox and Vaccination come at an opportune time. That by Dr. Woodward is of a purely popular nature, and, we doubt not, will do good service in combating the stupid, ignorant, and absurd outcry raised against vaccination by the Anti-Vaccination League.

Dr. Crisp's pamphlet is intended for both the non-medical and medical public; it gives a concise account of the history of vaccination, and of vaccine experiments upon the lower animals.

Dr. Vintras' pamphlet consists of a report, presented by him to the Brazilian Minister in London, in reference to the so-called animal vaccination, which the author believes is, in many cases, preferable to vaccination from arm to arm.

Dr. Chairou's *brochure* is a very interesting account, historical and otherwise, of variola and vaccination; it is written in the form of letters.

In the report on public health, which appears in this number, nearly all these works on hygiene are alluded to, and made good use of.

Dr Gould's large volume contains a really prodigious amount of statistical information, chiefly in relation to the physical condition of men of the ages liable to military service. The mean stature, weight, dimensions of various parts of the body, strength, respiratory capacity of men of different nationalities are given with a degree of comprehensiveness and completeness that leaves nothing to be desired. The only fault we have to find with this most valuable contribution to anthropological science is that the measurements are not expressed on the metric system, which is now almost universally adopted in works of that class.

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1. *Transactions of the American Ophthalmological Society.* Seventh Annual Meeting, New York, July, 1870. New York: John Meddle, 193, Pearl-street. 1871.

2. *Notes on the Dioptrics of Vision.* By R. E. Dudgeon, M.D.

The American Ophthalmological Society is doing good and practical work, as evidenced by the volume of their transactions now before us, which contains an admirable critique on recent publications, under the head of *Report on the Progress of Ophthalmology*

by Dr. Jeffries, of Boston, besides the minutes of the Society's meeting, and upwards of twenty separate essays by various authors on different subjects. Judging from this and other American publications we have from time to time noticed in this Journal, it would appear to us that there is springing up in America a school of ophthalmology, which bids fair to compete, and not unsuccessfully, perhaps, with the German School, more particularly now that the great German teacher v. Gräfe is dead.

Dr. Dudgeon's pamphlet, extracted from the *British Journal of Homœopathy*, purports to be "a demonstration of the dioptrical mechanism of vision, which rests more on experiments than on calculations." The author is of opinion that the figures and calculations of Donders, Cramer, and Helmholtz, respecting the lenticular system of the eye are erroneous and proceeds to correct them; but as his experiments were made on the eyes of bullocks removed from the body, and as he had "no instruments that would enable him to take very minute measurements," and as the experiments and calculations of the Continental authors just named were made on the human eye, and as they possessed every imaginable instrumental advantage, their conclusions will still probably be regarded as less erroneous than those of Dr. Dudgeon, who appears to believe in the adjustment of the eye, being partly brought about "by pressure of some of the muscles of the eye, causing increased convexity of the cornea."

Lectures on Aural Catarrh; or the Commonest Forms of Deafness and their Cure (mostly delivered at St. Mary's Hospital). By Peter Allen, M.D., Fellow of the Royal College of Surgeons, Edinburgh; Member of the Royal College of Surgeons, England; Aural Surgeon to and Lecturer on Aural Surgery at St. Mary's Hospital; Aural Surgeon to the Royal Society of Musicians; late Surgeon to the Metropolitan Ear Infirmary, Sackville-street. London: J. & A. Churchill, New Burlington-street. 1871.

We can recommend these lectures to such of our readers as are desirous of obtaining information on aural surgery, without having to wade through a mass of, no doubt highly valuable, but still very dry and uninteresting reading, such as is generally found in the text books on aural surgery. Dr. Allen has, we think, wisely restricted

his present series of lectures to the consideration of some of the commonest forms of deafness; the first six lectures are devoted chiefly to the methods of examining the auditory canal, membrana tympani, Eustachian tube and middle ear; the remaining six lectures are devoted to a consideration of aural catarrh in its various forms, its causes, effects, and treatment. The work contains frequent references to modern writers on aural surgery, and descriptions of Politzer's method of inflating the tympanum, and other modern achievements in this department; it is clearly and pleasantly written, and its pages are interspersed with valuable physiological and practical facts. The author's explanation of the improvement which takes place in the hearing of some deaf persons in railway carriages or elsewhere amidst great noise is particularly interesting and ingenious.

Lectures on Dermatology; a Synopsis of Diseases of the Skin. By ERASMUS WILSON, F.R.S., F.R.C.S. London: J. & A. Churchill. 1871. 8vo, pp. 146.

IN the year 1869 a chair of Dermatology, the first in Great Britain, was founded in the Royal College of Surgeons of England by the liberality of Mr. Erasmus Wilson, and in 1870 he inaugurated the chair with a course of lectures, which, reprinted from the *Medical Times and Gazette*, are now issued in an attractive form, under the above heading. Since the detailed descriptions of skin diseases are given in Mr. Wilson's large work, these lectures are simply intended as a conspectus of the whole subject of Dermatology, and may be briefly characterized as consisting essentially of definitions of the various diseases of the skin, with an enumeration of their varieties, and a prodigality of lengthy terms, calculated to deter the most zealous student. Fortunately, however, no one uses these dreadful polysyllables except Mr. Wilson himself. Thus, for example, we are furnished with 19 forms of eczema, 23 of erythema, and 13 of herpes, while *dermatoxerasia*, *acrochordon*, *cacotrophia*, *phalarotes*, *trichodyschroia*, *poliothrix* and *onychogryphosis* are a few of the many portentous words used, not as synonyms, but as substantive terms. This is the more to be regretted as Mr. Wilson has elsewhere protested against what he styles the "terminological innovations of modern nomenclators."

The nomenclature of the disease, commonly known here as

psoriasis, seems to be as great a puzzle in dermatology as corrosive sublimate is in chemistry, and its name has undergone many curious mutations.

A few years ago Mr. Wilson held that "next to precision of classification there is no one thing more needed in cutaneous medicine than precision and accuracy of nomenclature," and no one will be inclined to dissent from this opinion. Now, up to the fourth edition of his large work, he had called common psoriasis, lepra, applying the former title to a variety of chronic eczema. But, in the fifth edition, following Hebra, he ostentatiously relegates the term lepra to true leprosy, and adopts the name psoriasis for the common scaly disease. In the sixth edition, published in 1867, he discards lepra and psoriasis, and restores the ancient term alphos, yet in 1868 he again veers round to lepra, sometimes styling the disease lepra alphos, and in January, 1869, as the crowning change, he revives the term lepra græcorum, and apparently, since that time, no other change has occurred to him, for, in the present volume, this last designation is adopted. Orthography proves as sore a trouble as nomenclature. In the last edition of his large treatise, he still maintained the common mode of spelling technical terms derived from the Greek. But, in his "Lectures on Ekzema and Ekzematous Affections," 1870, he suddenly and enthusiastically alters the spelling of familiar words, and insists that we should write *ekzema*, *leichen*, *bouknemia*, *alkohol*, and so on. Yet such is the instability of his opinions, that while in these lectures under review, as they appeared in the *Medical Times and Gazette* of last year, he adhered to the Greek orthography, he now as suddenly abandons it, and reverts to the more usual terminology founded on the Latin tongue. It is time that such idle hair-splitting should cease, and it is useless to hope for a more fruitful study of skin diseases till all unnecessary and pedantic terms are banished from its vocabulary. For the reasons just alluded to, taken in connexion with a persistent refusal to acknowledge the parasitic nature of the diseases commonly so recognized, we think that Mr. Wilson leaves himself open to the imputation of retarding the study of his favourite specialty, and that, notwithstanding his learning and abilities, he has not quite fulfilled his own claim of having done his "best to move onwards with the times."

The Philosophy of the Bath, with a History of Hydro-Therapeutics, and of the Hot-air Bath from the earliest Ages. By DURHAM DUNLOP, M.R.I.A. Moffat & Co. 8vo, pp. 465.

WRITTEN by one who admires the bath system as ardently as he despises what he contemptuously terms the Drug School, the evident partizanship of the author runs through and mars the whole book. A long introductory chapter is devoted to a zealous diatribe against the Medical Profession in general, and "Drug Practitioners" in especial, in which, as is usual, some truth and much perverse error are to be found. The history of hydro-therapeutics and of the hot-air bath is fully given, and after a brief sketch of some elementary physiological principles, the curative action of the bath is treated in detail.

It is scarcely fair, however, to twit the profession now with utter ignorance respecting the therapeutical properties of changes of temperature, or to stigmatize the majority as "mentally incapable of appreciating" the beneficial effects which rightfully and admittedly belong to the bath, and we certainly do not think that Mr. Dunlop has made out a case for his concluding and very positive assertion, that "the disease has yet to be discovered to which the bath is not applicable."

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PART III.

QUARTERLY REPORTS.

REPORT ON MEDICINE.

By James Cuming, M.A., M.D.; Professor of Theory and Practice of Medicine, Queen's College, Belfast; Physician to the Belfast General Hospital; Examiner in Medicine in the Queen's University in Ireland.

DISEASES OF THE HEART AMONG SOLDIERS.

The Alexander Prize Essay, by Dr. Myers,[b] of the Coldstream Guards, contains some interesting information regarding the prevalence and character of cardiac diseases among soldiers. The author states that heart disease is more prevalent in the army than in the civil population—a fact which seems fully borne out by statistical returns. While the mitral valve is more frequently the subject of disease among the civil population, the aortic valves, according to Dr. Myers, are more frequently diseased in the army. This statement rests on inadequate evidence ; the number of instances adduced being quite insufficient to establish its truth. He regards the constriction produced by the uniform and accoutrements as the cause of functional derangement of the heart, especially among young soldiers. The functional derangement takes the form of irritability and excited action, which, if not allayed, brings about hypertrophy, and at a still more advanced period, according to the author, valvular disease and aortic aneurism. Dr. Myers states that he has found in cases of irritability of heart among soldiers that a more or less marked increase of dicrotism was present,

[a] The author of this Report, anxious that every contribution to Pathology and Practical Medicine should be noticed, will be glad to receive any publications on these subjects. If sent to correspondents of the Journal they will be forwarded.

[b] The Etiology and Prevalence of Disease of the Heart among Soldiers. The "Alexander" Prize Essay, by Arthur B. R. Myers, Assistant Surgeon, Coldstream Guards. London : Churchill. 1870

and this he regards as a valuable diagnostic sign occurring before any change can be detected by consultation or percussion.

Dr. Da Costa[a] has made a careful clinical study of irritable heart occurring among soldiers, and has arrived at conclusions which differ in some important respects from those of Dr. Myers. He observed cases of this kind in infantry, cavalry, and artillery, a proof that it was independent of the form of accoutrement. Out of 200 cases he found that it came on after fevers in 34, after diarrhœa in 61, after hard field service, particularly excessive marching, in 69 ; from wounds, injuries, rheumatism, scurvy, ordinary duties of soldier life, and doubtful cases in 36.

Hypertrophy of the heart was a frequent result in Dr. Da Costa's cases, but evidence of valvular disease was rare. What was noticed in some cases was a murmur of the character regarded by the author as a functional valvular one,[b] but this was an exceptional occurrence.

In the treatment of these cases Dr. Da Costa found that digitalis was of the highest value in pure cases of irritable heart. In slight cases it lowered the pulse, and made it steadier in about a week, and gradually led to permanent improvement. In severe cases its action was slower, but finally as markedly beneficial.

In cases in which there was reason to suspect the existence of hypertrophy its action was sometimes useful, but often doubtful or negative. The doses in which this remedy was administered varied, 10 drops of the tincture of digitalis or from $\frac{1}{60}$ to $\frac{1}{30}$ of a grain of digitaline being the average quantities.

Aconite administered two or three times daily, in doses of from one to two minims of a tincture of the root, three times as strong as that of the British Pharmacopœia, was remarkably useful in cases of decided hypertrophy, even causing, in the author's opinion, a decrease of the enlarged organ.

The veratrum viride had a better effect than aconite on irritable hearts, though not so good a one on cases of hypertrophy. It seemed to stand midway between digitalis and aconite.

Belladonna and atropia were particularly valuable in cases of irregular action of the heart. Rest in the recumbent position was found to be in the highest degree beneficial.

The author believes that he has established by his observations the connexion between functional derangement and organic change,

[a] American Journal of the Medical Sciences. January, 1871.

[b] See Report on Medicine in this Journal for November, 1869.

and ascertained the intermediate steps. As a contribution also to a more accurate knowledge of the effects of remedies on the heart, his paper will be found replete with practical interest.

MEDIASTINAL TUMOURS.

Dr. Franz Riegel[a] contributes an elaborate paper on the diagnosis and pathology of tumours of the mediastinum, and details an interesting case which was observed in the hospital at Würzburg.

Of 42 cases collected by Riegel, in which the nature of the tumours was ascertained, 33 were cancerous or sarcomatous, 4 fibroid, and 5 dermoid cysts. Virchow is of opinion that many of these tumours take their origin in a persistent thymus gland; others probably spring from mediastinal or bronchial glands, from glands in the root of the lung, from areolar tissue in the outer coat of the great vessels. Of carcinomatous tumours, medullary cancer is the most frequent.

Mediastinal tumours are met with oftener in the male than in the female sex. They occur most frequently in persons between 20 and 30 years. They may present no symptoms whatever for a long period, and when at length they appear the disease may make extremely rapid progress, and lead within a very short period to a fatal issue. In a case described by Jaccoud, the patient did not complain of anything till eight days before death took place. More commonly, however, the symptoms are developed gradually. Mediastinal tumours may grow upwards towards the neck or forwards, so as to destroy the sternum and ribs. In the majority of cases they grow inwards, compressing the lungs, the heart, the vessels and nerves towards the vertebral column. Very often a bulging will be observed in the thoracic wall corresponding to the tumour. When the tumour attains some magnitude a want of symmetry in the sides of the chest becomes manifest. Pulsation is sometimes observed in the bulging part. The respiration is accelerated, its type varying with the size and situation of the tumour. Percussion establishes the presence of dulness and increased resistance when the tumour is situated near the thoracic wall. The vocal fremitus is usually diminished, but in the case of solid homogeneous tumours may be increased. Mensuration usually reveals the existence of a difference between the sides of the chest. For the most part no respiratory sounds are to be

[a] Virchow's Archiv., Band xlix. Heft. 2.

heard over the tumour, but weak or bronchial breathing may be audible.

In the progress of tumours effusions commonly take place into the pleural cavity and into the pericardium. The cardiac impulse is weakened, the sounds less intense, and the organ itself is often displaced. The superior cava is often compressed with consequent dilatation of its tributaries and œdema of the face and upper extremities. Arteries offer greater resistance to pressure, but they become narrowed with consequent inequality of the radial pulses. Enlargement of the thyroid with some degree of exophthalmus have been observed, although rarely.

The trachea, the larger bronchi, sometimes undergo compression, the œsophagus also, causing dysphagia, the vagus and recurrent nerves with dyspnœa, alteration of the voice, or even although rarely aphonia. Pain to a greater or less extent is generally present. Enlargement of the axillary, or supra-clavicular glands, is also a sign of some value as establishing a probability of a malignant character of the intra-thoracic tumour. With the progress of the disease dyspnœa becomes more considerable; cough and expectoration are observed, and pain referred to the arm, neck, or back, or intercostal neuralgia may be present.

On the subject of the diagnosis of mediastinal tumours Professor Skoda[a] has contributed some observations.

The tumour is probably malignant, if the patient is old and cachectic, and if the glands in the neighbourhood of the sterno-mastoid or in the axilla are enlarged. Considerable and extensive interference with the function of the organs in the thoracic cavity point in the same direction, although these may also be to a great extent perverted by mere pressure or displacement.

Non-malignant growths are less common, and are slower in producing disturbance of the system at large. Pulsation may be noticed when the tumour is seated on an artery, or may be propagated from the heart. The non-expansive character of the pulsation and the usual absence of bruit serve to distinguish tumours from aneurism; but it is often a matter of difficulty to establish the diagnosis. Pericardial exudation may be mistaken for mediastinal tumour. Acute pericarditis, however, is accompanied by fever, and occurs in connexion with some general disease as

[a] Allg. Wien. Med. Zeitung, No. 20, et seq., 1870. Prager Vierteljahrschrift, 1871. Band. 1.

acute rheumatism, Bright's disease, &c. Chronic effusion into the pericardium is also secondary to other affections.

Pleuritic effusion resembles some mediastinal tumours in producing bulging of the chest, dulness on percussion, diminution or absence of the respiratory sounds, &c. The differential diagnosis becomes especially difficult when effusion in the pleura co-exists with tumour. If the tumour is connected with leucocythemia there is generally sufficient evidence elsewhere of the implication of the glandular system.

TREATMENT OF COMMENCING CHRONIC DIARRHŒA IN YOUNG CHILDREN.

Dr. Eustace Smith[a] has contributed some valuable suggestions on the management of diarrhœa in young children. In incipient cases of this affection, in which the motions contain undigested food, farinaceous food will often be found to disagree, and the use even of milk must sometimes be discontinued. The form of farinaceous food most likely to suit children, the subject of this affection, is Liebig's. In cases occuring in children of from eighteen months to two years, in whom it is found that milk is not well borne, Dr. Smith suggests the following scale of diet:—

"1*st Meal.*—One teaspoonfull of Liebig's food for infants (Mellin's) dissolved in four ounces of milk and barley-water (equal parts).

"2*nd Meal.*—Six ounces of beef-tea, of the strength of a pound of fillet of beef to the pint.

"3*rd Meal.*—Six ounces of fresh whey containing a tablespoonful of cream.

"4*th Meal.*—The unboiled yoke of one egg, plain, or beaten up with a tablespoonful of cinnamon water, a little white sugar, and fifteen drops of brandy.

"5*th Meal.*— Same as the first.

"In this dietary the first and the fifth meals contain a small quantity of milk. If that be found not to agree, the food may be dissolved in barley-water alone, or diluted with an equal quantity of veal broth, or veal broth alone may be given. In any case the quantities recommended should not be exceeded; for it is wise, at any rate at first, to be sparing rather than liberal in regulating the allowance of food. It is better that the child should be hungry than overloaded, and so long as the stools retain their pasty character it is evident that the food taken remains in great part undigested."

[a] Practitioner, July, 1870.

As regards medicinal treatment after an aperient dose of rhubarb and soda, Dr. Smith recommends five to ten grains of bicarbonate of potash with an aromatic to be given several times daily, with, if the stools are loose and are passed frequently, two to three grains of the subnitrate of bismuth to each dose, and if there is much straining a drop of laudanum in addition. It is of importance that the dose should be given an hour or an hour and a half after each meal. There is a strong tendency in children to acid fermentation of the food, and when administered at this period, any excess of acid left at the end of digestion will be at once neutralized.

It is desirable that the child should be warmly clothed, and should wear a flannel bandage round the abdomen. The functions of the skin must also be attended to.

PERCUSSION OF THE STOMACH.

Dr. Wagner[a] has investigated some points regarding the situation of the stomach by examining it by percussion when filled with carbonic acid gas, a plan suggested by Frerichs. The gas is developed by the administration of a little tartaric acid, and afterwards a little bicarbonate of soda, or by a glass of some effervescing water. By this expedient the stomach becomes distended, and can then be readily distinguished from the neighbouring organs by the tympanitic sound produced by its percussion. The magnitude and boundaries of the liver and spleen are also more readily ascertained by percussion when the stomach is distended by gas, and Wagner believes that their palpation is also facilitated.

His conclusions as to the normal position of the stomach agree in the main with those of Luschka already noticed in this journal.

TREATMENT OF OBSTINATE VOMITING BY IODINE.

In cases in which obstinate vomiting occurs after the administration of aperients, Caspar and Bierbaumb[b] have found that the administration of iodine as recommended by Rademacher has been followed by excellent results. Thirty drops of the tincture of iodine, with a scruple of tragacanth, are added to eight ounces of water, and of this mixture a tablespoonful is to be taken every hour. These observers have found that gastric pain as well as vomiting subsides under the use of this remedy.

[a] Prager Vierteljahrschrift, 1870, Band 3.
[b] *Ibid.*

CO-EXISTENCE OF TWO ERUPTIVE FEVERS.

Professor Thomas,[a] of Leipzig, details four cases, in each of which two eruptive fevers co-existed. In one scarlet fever appeared in a patient, the subject of varicella; in the second varicella appeared during the progress of scarlet fever; in the third measles appeared in the course of varicella; and in the fourth varicella occurred in the course of measles.

The character of both the measles and the scarlet fever was very mild in the cases in which these affections were preceded by varicella, but in the cases in which varicella occurred secondarily the other maladies were notably more severe.

The author has collected a number of cases recorded within the last few years in which the co-existence of two eruptive fevers has been ascertained. The combinations present, besides those already mentioned, have been measles succeeded by scarlet fever, scarlet fever succeeded by measles, measles succeeded by small-pox, and *vice versa*, scarlet fever and small-pox.

INFANTILE PARALYSIS.

Mr. Barwell,[b] Surgeon to Charing Cross Hospital, discusses the interesting and important subject of infantile paralysis. He regards this affection as being independent of any recognizable change in the nervous centres. It is purely peripheral, affecting the ultimate fibrillæ of the nerves distributed to the muscular element. The cause is to be sought for in some derangement of the relationship between the muscular and nerve fibres, perhaps of an inflammatory character, perhaps merely chemical or nutrient. He combats the view that the essential element in the disease consists in spinal congestion, and enumerates the following characters which point to a radical difference between the two forms of affection:—

"Spinal Congestion.	"Infantile Paralysis.
"Paralysis incomplete, always at first and frequently throughout the case. The paralysis tends, however, to deepen.	"Nothing can be more utter and entire than the loss of power in the very commencement; it tends, however, to get less marked, to lighten.
"Generally paraplegic in form at the end, if not in the beginning. Usually one leg is attacked first, then the other. The malady becomes more extensive as it goes on.	"Paraplegic form is not usual in the commencement, still less afterwards. When the paralysis has attacked one part it never afterwards affects another. This malady is most extensive at its commencement.

[a] Jahrbuch für Kinderheilkunde, November, 1870.

[b] St. Thomas's Hospital Reports, Vol. i., 1870.

"Spinal Congestion.	"Infantile Paralysis.
"Tingling in fingers and toes.	"I have never been able to detect the slightest sign of pain or discomfort. There may be a little increase of sensibility to touch, but this is doubtful.
"Shortness of breath is not infrequent.	"No alteration ever observed in the breathing function.
"Fluctuations towards better and worse occur in all cases; sometimes these are rather frequent.	"No change backward or forward ever occurs; if the patient gains a step he does not lose it again.
"There is a decided tendency to relapse, and this even after the patient appears to have got well.	"No tendency to relapse. The stroke being given, the evil does not strike again, or aim at another part.
"Although it is generally supposed that reflex irritability is increased, Dr. Radcliffe thinks that, if at all altered, it is diminished rather than increased.	"Reflex irritability is utterly annihilated, so that there could not be any doubtful opinion by different authorities.
"No wasting of paralysed muscles.	"Extremely rapid wasting of paralysed muscles, which sets in very shortly after the first attack.
"No marked impairment in paralysed muscles of electro-contractility.	"Remarkable impairment of electro-contractility; indeed, total loss of response to Faradization.

"The above differences are very marked and so important that the first six alone appear to me sufficient grounds for separating, nosologically, infantile from spinal paralysis; but it is chiefly on the last three that I would just now direct attention.

"1. There is no central paralysis in which, completely and from the moment of attack, reflex contractility is so utterly banished.

"2. There is no form of central paralysis in which the muscles behave to different galvanic currents as they do in this disease.

"3. There is no form of central paralysis in which the muscles lose temperature, waste, and shrivel, as they do in this disease, without becoming rigid."—*St. Thomas's Hospital Reports.*

As regards treatment Mr. Barwell lays much stress on the value of energetic measures being adopted at an early period of the disease, for the purpose of preventing muscular and nervous degeneration, which, he says, sets in rapidly. This has been denied by Volkmann[a] and Steudener, who have repeatedly examined small pieces of muscles which had been completely paralysed, and found the primitive muscular bundles to exhibit granules, the nuclei, and the interstitial fat to be somewhat increased in amount, in some cases the muscular fasciculi reduced in bulk, but no where distinct fatty degeneration. What Mr. Barwell has found most useful is the constant current, while the electricity ought to be of high

[a] Sammlung Klinischer Vorträge—Jahrbuch für Kinderheilkunde, Sept., 1870.

tension but small quantity, and less than 50 cells are insufficient. The induced current is rather injurious than otherwise.

SYPHILITIC AFFECTIONS OF THE NERVOUS SYSTEM.

Dr. E. L. Keyes, of New York, has published, in a separate form, a communication which appeared in the *New York Medical Journal*, November, 1870, on the subject of syphilis of the nervous system. The paper is founded on the observation of thirty-four cases. Of these the majority were under the care of Dr. Van Buren, and they have been all observed in private practice. This fact is of importance, because the condition of this class of patients is, in most instances, capable of being ascertained from time to time, a circumstance of the greatest value with regard to maladies of which the course is protracted through a series of years. The following conclusions have been drawn by the author from his study of those cases:—

"1. That nervous symptoms depending upon syphilis may arise within the first few weeks after an infecting chancre, or at any period later during the life of the individual.

"2. That it is presumable, from the study of published autopsies, that the earlier a nervous system (paralytic or otherwise) occurs, the less likely is there to be any material lesion which an autopsy can reveal; and that in a given case there exists no constancy of relation between the nature, the situation, and the severity of the lesion, and the nature, situation, and severity of the nervous symptom, to which that lesion may give rise.

"3. That cerebral congestion is probably the pathology of many of the earlier nervous syphilitic symptoms.

"That syphilitic hemiplegia occurs, as a rule, without loss of consciousness, even when the attack is sudden; but that the paralysis usually comes on gradually, the patient being under forty years of age, and having had fixed constant headache for some time before the attack.

"5. That mydriasis, existing alone, or with other nervous symptoms, without positive disease of the eye, is presumptive evidence of syphilis.

"6. That paralyses of single muscles, or sets of muscles, are frequently syphilitic.

"7. That syphilitic paraplegia generally comes on gradually, often without any local symptom to call the patient's attention to the injured portion of the cord, and that it is rarely complete. That the bladder almost always suffers more or less, and calls for special local treatment. That paraplegia may be developed as a symptom of inherited syphilis.

"8. That syphilitic epilepsy usually occurs after thirty, in patients

who have not had epilepsy in early life. That headache is liable to precede the attacks. That the convulsions occur often, many in quick succession, the intermission between the series of attacks being comparatively long, but that, during this period, headache or other nervous symptoms exist and become aggravated, contrary to what obtains in idiopathic epilepsy. That syphilitic epilepsy is liable to be associated with, or followed by, some form of paralysis.

"9. That aphasia is often associated with the intellectual disturbances caused by syphilis.

"10. That loss of memory is a common nervous symptom of syphilis, as are also all forms of mental disturbance—from mild hallucinations and illusions up to actual insanity, and all these without any necessary accompanying paralysis.

"11. That inordinate emotional expressions are often associated with the mental weakness caused by syphilis.

"12. That care must be taken to distinguish certain symptoms caused by gout, from the same symptoms owing their origin to syphilis.

"13. That the prognosis is better as a rule for nervous symptoms caused by syphilis than for the same symptoms depending on a lesion equal in extent, caused by another malady of the nervous centres; but that, after the arrest of the disease, an indelible impression is often left upon the nerve-tissue, which manifests itself by impaired function, and which treatment cannot overcome.

"14. That the iodide of potassium pushed rapidly to toleration, unless the symptoms subside before that point is reached, is the main outline of treatment. That mercury, used at the same time, or alternated with the iodide of potassium, is often of great value in protracted or inveterate cases; and that tonics, change of air and surroundings, frequently influence the effect of treatment in a marked degree, and may become essentials to success."—*New York Medical Journal.*

Dr. Moxon[a] has detailed the history and *post-mortem* examination of an interesting case of syphilitic disease of the spinal cord. The following observations on the recognition of the nature of syphilitic nervous disease will be found to possess much interest:—

"It has already been remarked by an able writer on syphilitic disease of the nervous system, that in the course of these affections the symptoms produced show two distinct kinds, in the important point of duration and consequences. Some symptoms come suddenly and disappear with a like quickness. Others arise slowly, and increase gradually, and persist tenaciously; often altogether refusing to decline, although the proper

[a] Guy's Hospital Reports, 1871.

specific remedies be fully employed with results completely satisfactory as to all accompanying manifestations of the syphilitic cachexia.

"It is interesting to remark that the varieties of paralysis which in syphilis suddenly come and go are such as commonly appear in a 'functional' form from other causes. These are especially hemiplegia, and the various kinds of ocular paralysis. Passavant, in the paper cited, advances a hypothesis in explanation of these transient paralyses; he thinks they may be due to such disease thickening the arterial coats, as shall narrow the channel of the artery and impede the flow in it, so that in a casual state of weakness of the heart's pressure, the force shall not be enough to carry blood by the obstruction. And hence, during such weakness, the part of the brain supplied by that artery shall be starved and palsied, until the heart recovering due force the thick artery yields again, and circulation and, with it, function are restored to the affected region of the brain. This idea is ingenious, and it certainly is true in support of this view or some one like it, that the transient paralyses affect those parts of the brain whose arteries are especially liable to disease, namely, those supplied by the middle cerebral and basilar arteries, vessels which we so constantly find to be diseased.

"On the other hand, the nerve affections in syphilis which prove permanent are those which point to direct syphilitic changes in the nervous substance, and among these paraplegia is already recognized (by Zambaco and others) as holding a conspicuous place; along with changes in the retina, choroid, or iris, which can be directly seen to be unaffected by remedies when once their destructive action has taken effect, and with evidence of change in the cortex of the brain. For all these, though relieved and checked by antisyphilitic medicines, yet too often are not entirely removed, and show unhappy proneness to recurrence.

"It has been a matter of surprise that this obstinacy should characterise the results of syphilis when mercurials and iodides have the power of overcoming the symptoms of syphilis. But reference to the nature of the gummatous change, as shown in the figure accompanying this paper, will show sufficient reason for the non-recovery of the nerve-tissues. The fact is that when what is called syphilitic deposit occurs in a part, that word *deposit* is apt to mislead as to the real nature of the change; it is not a lodgment of syphilitic matter among the proper elements of the part, it is a change of those elements into the syphilitic matter. They no longer exist in the 'deposit.'"—*Guy's Hospital Reports.*

REPORT ON OBSTETRIC MEDICINE AND SURGERY.[a]

By GEORGE H. KIDD, M.D., F.R.C.S.I.; President of the Dublin Obstetrical Society; Honorary Fellow of the London Obstetrical Society; Corresponding Member of the Gynecological Society of Boston; Obstetric Surgeon to the Coombe Lying-in Hospital.

WE have before us for the present report a series of papers of very great interest in reference to malignant diseases of the uterus, and also one detailing a series of cases of that recently recognized condition—pelvic hæmatocele.

I. MALIGNANT DISEASES OF THE UTERUS—PATHOLOGY.

Mr. Henry Arnott,[b] Surgical Registrar to the Middlesex Hospital, communicated on 3rd May, 1870, to the Pathological Society of London, a very interesting and valuable report in a tabular form, of 57 cases of cancer of the uterus, the results of which he sums up as follows:—It is rarely that opportunities of examining such cases are obtained, as the patients are not generally kept in hospital for any length of time, but in the Middlesex Hospital there are special cancer wards where the patients generally remain till their death, and Mr. Arnott had an opportunity of noting 136 cases, of which 75 died in the hospital, and 57 autopsies were held:—

"Amongst English writers, Paget gives no very special description of the varieties of uterine cancer. Epithelioma is mentioned as a common form of the disease, but so too are scirrhus and encephaloid. No proportion between the two forms is given. Walshe does not recognize epithelioma as a distinct form of cancer, and hence divides all uterine cancers into scirrhus and encephaloid. Tanner, in a paper containing notes of ninety-two cases of this disease, rarely mentions the existence of secondary growths. As to the form of cancer present, the microscopic appearances are not given in detail, but in thirty it is said to be impossible to determine the point, in forty-seven it is styled medullary, in seven scirrhus, and in eight epithelioma. This writer says, 'Medullary cancer is very much more frequent than any other variety of malignant

[a] The author of this Report, anxious that every contribution to obstretric medicine and surgery should be noticed, will be glad to receive publications on the subject. They may be sent to the publishers of the Journal through their correspondents.

[b] Cases Illustrating Certain Points in the Pathology of Cancer of the Uterus. By Henry Arnott. Transactions of the Pathological Society of London. Vol. xxi. 1870. P. 281.

disease of the uterus. . . . Cauliflower excrescence, or epithelioma, is also a rare affection. Just as seldom, I believe, an inveterate form of ulcerated epithelial cancer of the lips or interior of the cervix falls under observation.'

"In like manner Lebert, writing on the same subject, says, 'Le véritable cancer est bien plus fréquent dans l'utérus que le cancroide, que nous n'avons rencontré que 6 fois sur 45.' Of these forty-five carefully examined cases, thirty-nine were said to be true cancer—either scirrhus or medullary, or something between the two.

"Cornil and Ranvier, writing last year, mention the fact of epithelioma being not uncommonly met with in the uterine neck, but give no statistics as to the comparative frequency of the different forms of cancer occurring in this region.

"Klob states that 'epithelioma of the uterus is a comparatively frequent new growth, and it is met with in two forms,' &c., &c.; that the 'cancroid disease appears first in the vaginal portions of the uterus as a papillary hyperplasia,' and that 'the early stage of simple papilioma can hardly be distinguished from cancroid.'

"And finally, Foerster says, 'The epithelial cancer is, as above mentioned, the usual form of uterine cancer; in fifty-two cases of uterine cancer examined by me, forty-two were of this nature.' A comparison of the contradictory statements of two such distinguished observers as Lebert and Foerster (both of whom seem to attach the same meaning to the terms employed), is sufficient to show the great need of further careful observations on the subject. None of the writers quoted give the proportions in which secondary cancerous growths occur in their cases.

"Turning to the work already published in this Society's 'Transactions,' one finds nineteen cases recorded, of which only fifteen were examples of primary uterine cancer. The microscopic structure is mentioned in only eight of these cases, four being apparently true cancer, three epithelioma, and one a form of sarcoma, in which spindle cells predominated. As to secondary growths: in eight the fact is not alluded to, in two it is said that there were none, in five the lymphatic glands were implicated, and in five growths in remote viscera were found—viz. (1) in the peritoneum and mesentery; (2) in the ovary, liver, lungs, pleura, and bladder; (3) in both ovaries; (4) in the lungs, and (5) in the liver.

"In the appended table of cases, observed by myself, it will be seen that of the whole number of autopsies held (57) there were no secondary growths in 34, the lymphatic glands were involved in 20, and in 11 the viscera contained secondary growths. Of these last the disease was met with in the ovaries 5 times, in the liver 3, in the lungs 2, in the heart 1, in both breasts 1, and in the peritoneum 1. Further, a careful microscopic examination of the parts was generally conducted, but in only 22 was the structure sufficiently clearly made out to be safely recorded. Of these,

true cancer was present 12 times, epithelioma 8 times, and spindle-cell sarcoma twice. A point of much interest to determine is the relation which exists between the microscopic structure of the malignant uterine growth and the implication of remote viscera or lymphatic glands. The number in the present table are of course of little use in helping towards a solution of this question. The results, as far as they go, are as follows :—In both cases of spindle-cell sarcoma, the disease appeared elsewhere also, in one case in the pelvic glands, and in the other in these glands and in the lungs. Of the eight epitheliomata, the taint extended in three instances—viz. (1) one ovary, and pelvic and lumbar glands; (2) the peritoneum (broad ligament); and (3) the lumbar glands. While of the twelve cases of true cancer, in nine instances the disease spread to other parts, pelvic or lumbar glands being affected in seven, one or both ovaries in four, liver in one, and heart and lungs in one. It is proper to mention here what special structure is indicated when the terms sarcoma, epithelioma, and true cancer are employed. The distinctions were usually sufficiently well marked in the cases noted, although it is often confessedly difficult to draw the line between these varieties of malignant disease. By the term spindle-cell sarcoma, as here employed, I mean a structure made up of densely packed cells having a spindle shape, being usually arranged in a tolerably regular manner, and containing generally single (rarely two) comparatively large oval nuclei. Owing to the manner of grouping of the cells, the spindle outline was usually difficult to make out save at the edges of a thin section where the fine-pointed extremities bristled out, or in detached cells floating about the field. In neither case was anything like encapsulation observed, either in the uterine affection or in the nodules in the lungs or glands. The sarcoma growth in this respect followed what I believe to be the general rule, although I am aware that sarcomatous tumours are thought by many to be usually invested by a capsule of some kind instead of freely infiltrating round tissues.

"By epithelioma, I imply an accumulation of ordinary or hypertrophied epithelial scales in an unnatural position, sometimes accompanied by 'nests,' or 'globes épidermiques,' although not necessarily so distinguished, and with usually a very disorderly clustering of the scales, which are otherwise disposed with cohering edges. In some cases a section carried through the mucous membrane of the cervix close to the ulceration showed hyperplasia of the epithelial elements upon and between the papillæ with infiltration of the same elements amongst the deeper structures, other sections, from a more diseased portion of the same uterus, exhibiting only the confused heaps of epithelial cells just described, with much broken-down oily or granular débris.

"Under the head of true cancers are included those cases in which a structure more or less closely resembling that of a scirrhous breast is met

with—viz., an alveolar fibrous stroma, in the interstices of which float, in a clear fluid and with no visible intercellular material, cells of varying shape, but all approximating somewhat to the squamous epithelial type, and containing usually only one large oval nucleus with bright nucleolus. In many cases the fibrous stroma, instead of forming a dense net-work, was visible only as a thin streak here and there, and in these cases the varied shapes of the cells and the absence of the intercellular substance stamped the cancerous nature of the growth. The cells occasionally contained multiple nuclei. In some places I thought that I could trace a development of this cancer structure from surrounding 'adenoid,' or lymphatic-gland-like material, the cells of the new growth taking the place of the small nuclei in the fibrous stroma of this structure; but more generally the cancer seemed to be splashed in—so to say—amongst healthy uterine tissue.

"A point of much moment to the practical surgeon is suggested by a careful comparison of the various columns in the table. Whatever may be the degree of malignancy attached to the several varieties of histological formations, these cannot apparently be surmised from the mode of invasion and progress of the disease as revealed by digital examination and with the aid of the speculum, at all events in the latter stages of the disease. It will be noted that in nearly every case the seat of the disease is the same. The os and cervix are more or less completely destroyed, and the foul ulcer resulting includes the upper part of the vagina. In more severe cases the floor of the bladder is invaded and perhaps freely perforated, whilst even the rectum may be opened into the vagina, the uterus being itself sometimes almost wholly consumed in the general havoc. In one remarkable case the os and cervix remained free, whilst the whole body of the uterus was destroyed by cancer. Separate nodules of the new formation were rarely met with imbedded in parts of the uterus at a distance from the ulcerated portion.

"Whilst this is true, however, of the stage of the disease met with in the dead-house, it is a question of far greater importance how far it is true of the early and possibly remediable stages. From our experience of the comparative malignancy of the same morbid processes in other parts of the body, we might expect to be able to deal far more satisfactorily with epithelial disease attacking the uterus than with true cancer in the same position. It would be extending the present subject beyond the scope of the object of this Society to enter upon this question here. It would be necessary in its consideration to take into account the duration of the several diseases, the precursory symptoms, the presence and amount of sanguineous discharge, the extent and fixation of the uterus, and all the other signs which guide us towards the early diagnosis of uterine cancer. In this inquiry these tables would be of but little use. So far as they show the average duration of the several forms of disease, it may be

remarked that in true cancer this was found to be 53·8 weeks, and in epithelomia 82·7 weeks. One cannot, of course, draw any conclusion from only two cases of sarcoma, although it is worth noting that both of these patients lived for a very considerable period after complaining of the first symptoms of the disease.

"It seems, no doubt, at first sight, singular that, with such varied anatomical processes, the naked eye appearance should be so similar. I believe that this is to be accounted for by the fact that the neck and mouth of the uterus, besides being extremely vascular, are subject to constant motion, and are very largely supplied with lymphatics. It is not surprising that any morbid infiltration tending to rapid cell growth and early decay should lead to extensive ulceration on a free mucous surface in constant friction against an opposed similar surface. Such irritation would be certain in all cases to set up inflammatory processes in their immediate neighbourhood, and the naturally irritating secretions of both uterus and vagina, tainted by the new addition, would speedily cause the spread of such malignant destruction in the manner witnessed in almost all of these cases.

"The practical conclusions which these tables suggest are—

"1. That in order to advance our knowledge of the pathology (and therefore of the rational treatment) of malignant disease of the uterus, a careful microscopic examination should be made of the morbid structures in every such autopsy, where this is possible, and special care taken to note the existence of any secondary focus of the disease, either in lymphatic glands or in remote viscera.

"2. That as regards the form of disease usually present, these and former observations recorded in the 'Transactions' of our Society tend to show that whilst true cancer is the most common disease—the structure ranging from one almost purely cellular to a tissue exactly resembling the firmest portions of a scirrhous breast—that epithelioma is also very frequently met with (in the proportion of nearly three to four), and that well-marked sarcoma is to be found affecting the uterus as well as other parts of the body, although it does not seem to have been hitherto looked for in this locality.

"3. That outbreaks of this disease in neighbouring lymphatic glands or in remote viscera are comparatively frequently met with, and furnish a strong argument against rash operative interference, with a view to extirpating the disease.

"4. That these secondary foci of disease being greatly more common with true cancer and sarcoma than with epithelioma, the microscopic examination of the morbid structure becomes of much importance in determining upon a line of treatment."

In a paper of great interest, Dr. Lusk[a] reviews the present state of the pathology of uterine cancer. After speaking of the histology of the healthy uterine structures, and of the various forms of cancer, the author proceeds to discuss the pathogenesis of these structures:—

"*Pathogenesis.*—The question as to the origin of these tumours is one of the most puzzling riddles in Pathology. Blastemists and cellular pathologists find here the favourite field on which to fight their battles.

"The blastemic theory, born in Germany, but soon after naturalized in France, maintains that the first step in the formation of these tumours consists in the exudation into the tissues of a special amorphous liquid (blastema). Later, certain atoms, or constituent molecules contained in the liquid unite and form the cancerous elements. This theory, derived from Schwann's doctrines concerning cell-development, is the basis of M. Robin's doctrine of 'genesis by substitution.' In embryonic life, according to M. Robin, the vitelline spheres, resulting from the continual segmentation of the ovum, are not directly transformed into the cells of the blastodermic membrane, but first liquefy and then become replaced by newly-formed anatomical elements. This is what he terms 'genesis by substitution.' In the same way, in certain tumours, the normal elements disappear, and are replaced by the elements which constitute the new tissue; only in this case the blastema is not visible, the new element immediately usurping the place of the old, in proportion as the latter disappears. The theory of blastemas hardly deserves the obloquy that has been heaped upon it by the *omnis cellula e cellulâ* school. The evidence of the transmutation of one cell into another is certainly not complete. M. Robin denies that this ever takes place. His doctrine of genesis simply affirm that, in certain cases, formed elements make their appearance in seemingly clear, structureless fluids, the process of the formation of these elements, previous to their attaining a certain stage of growth, being inaccessible to our present means of observation.

"In Germany the blastemic theory never gained universal acceptance. Even when it was the reigning doctrine, there were embryologists who declared themselves unable to trace the origin of cells from blastemas in the embryo, and who regarded the extra-cellular origin of animal cells as no less improbable than the *generatio æquivoca* of organisms. Remak (1852), extended the doubt to the formation of cells in pathological tissues, and ventured the conjecture that one day they would be found to be directly derived from the normal tissues of the body. Finally, Virchow

[a] An Inquiry into the Pathology of Uterine Cancer, with special reference to the Significance of Certain Symptoms. By Wm. T. Lusk, M.D. New York Medical Journal, September, 1869.

propounded his doctrine of the continuous development of cells from one another. In the connective tissue and its equivalents, he declared that he had found the common stock of germs (keim stock) of the body, to which, as a general source, we may directly trace the development of new formations. The formation of a neoplasm, according to Virchow, is ushered in by the repeated sub-division of the connective-tissue corpuscles, the segmenting process continuing until there result whole rows of small cells. These are the formative cells of the succeeding new creation. At this period, however, we can discover no characteristic by which we determine their destiny. They are like embryonic cells, of which those that are to become brain behave precisely like those that are to become muscle. Then follows the stage of differentiation, when these formative cells become developed, 'according to the particular nature of the irritation to which they are exposed,' into widely different tissues. Thus, in one case, they may pass by gradual transitions into cylindrical or pavement epithelium; in another, they may secrete a certain quantity of intercellular substance, and give rise to connective tissue; or, still again, a part of the cells may become developed into connective tissue, while the rest may pass into epithelium. The reciprocal arrangement of these elements may vary, and we may have epithelium contained in alveolar spaces; or the neoplasm may present a remarkable resemblance to physiological organs, as the glands. In this way the cancerous structure could be supposed to originate. The success of the new doctrine was astonishing. Pathologists greeted it with cordial approval. There were few doubters, and they were not held in very good repute. Yet *à priori* this cellular Darwinism was not one whit less mysterious or repugnant to reason than the '*generatio æquivova*,' all had united in rejecting. The question was, however, one of fact, and certainly the evidence seemed to sustain the new theory. Everyone verified Virchow's observations. Not to see things as he saw them was a sign of blindness or conceit. For a time the triumph was complete, and it is only lately that symptoms of reaction have set in. Some recent observers, however, claim that the spherical nuclei resulting from the continued division of the connective-tissue cells can everywhere be distinguished from epithelial nuclei, and have failed to discover those transitional forms by which, in diagrams, the intimate connexion between the two has been established. Thiersch argues further, that 'it is not probable that connective tissue can produce epithelium under pathological conditions, if this capacity be wanting under normal conditions,' a capacity he denies to have been proven. Moreover, in the embryo, epithelium and connective tissue are derived from different layers of the blastodermic membrane, and both are present before these layers come into contact with one another.

"Cornil has carefully studied the early stages of uterine cancer, selecting for the purpose either a tumour at its *début*, or the peripheral

(more recent) parts, or the secondary granulations in the serous membranes. In his medullary form, he tells us, a framework of new connective tissue exists from the first. In this, microscopic alveoli, isolated from one another, form, and become the receptacles of epithelial nuclei. These nuclei increase in number, and become surrounded by an amorphous granular atmosphere and cell-membrane. The vessels traverse the spaces between the alveoli, but not in immediate contact with the cells, a third layer of connective tissue intervening. At this period the tumour is hard, and has sometimes been termed scirrhus. Later, however, the cells increase, and the alveoli enlarge. Then the connective tissue of the alveolar walls undergoes fatty degeneration, and two or three alveoli communicate together, so that a softening takes place. Cornil had an opportunity to examine two cases before ulceration occurred, and states that in both, the first development of the neoplasm took place beneath the mucous membrane. He explains the presence of the nuclei and cells, in parts devoid of the same, by the French doctrine of genesis. Virchow makes the distinction between malignant growths and simple papillary or granular tumours to consist in the point that, in the former, the epithelium is derived not from pre-existing epithelium, but from connective tissue. Cornil, however, was unable to obtain preparations demonstrating the derivation of the epithelial constituents from the hypertrophy and hyperplasy of the connective-tissue cells. Indeed, they everywhere appeared very distinct and without intermediate forms. Thus, the connective-tissue nuclei were small, measuring from $\frac{1}{6000}$ to $\frac{1}{3000}$ of an inch in diameter, while the epithelial nuclei were ovoid and measured $\frac{1}{2800}$ of an inch in diameter. Moreover, the smaller nuclei appeared solidly united to fibres of connective tissue, while the larger were entirely free, and could be easily pressed from the alveolar cavities in which they were contained.

"A considerable number of pathologists, at the present day, while holding to the doctrine of continuous development, have adopted Remak's proposition that 'epithelium can only come from epithelium.' Waldeyer believes that there is no primary form of uterine cancer not derived from the glandular constituents of the organ. 'If we consider,' he says, 'the folds and depressions of the mucous membrane lining the cervix uteri upon which the glandular tubules open, from which the cancerous collections are most commonly derived, we shall not find it strange that we so seldom discover, upon transverse or vertical sections, the direct connexion between the cancerous masses and the gland-orifices. But, by taking pains, and keeping close to the edge of the neoplasm, it is possible to make preparations showing that such connexions really do exist.' Waldeyer has a specimen with the external orifice of the gland still preserved, while the lower extremity and the lateral offshoots communicate with larger irregular collections of epithelial cells. To these collections

he gives the names of 'cancerous bodies' (Krebs Körper), and states that they are larger in the uterus than elsewhere. The epithelial cells are usually of the pavement variety, and occasionally cylindrical. In those forms of cancer in which the glandular arrangement was lost, Waldeyer found hardly any except large round cells, even where, in normal conditions, cylindrical cells had been present. Generally he found the cells embedded in pure connective tissue, though sometimes they were contained in the muscular substance of the uterus. The stroma, in the former case, is derived partly from proliferation of the connective tissue surrounding the glandular acini, and partly from that of the organ itself. According to the predominance of epithelium or stroma, and the departure more or less from the glandular type, we would have the different varieties constituted. Analogous observations, tending to support the epithelial origin of cancer, have been made in other parts of the body, as the breast, stomach (pylorus), kidney, liver, testicle, and skin (cancroid). Billroth, once an advocate of the 'cellular theory,' regards a cancer as an epithelial neoplasm possessing generally a glandlike arrangement, to be distinguished, however, from simple glandular hypertrophy in its departure from the *original* gland-type.

"Thiersch, Billroth, and Waldeyer, regard the epithelium of serous membranes and of vessels as elements belonging to connective tissue, to be shut out, therefore, from 'true' epithelial tissues, which alone are capable of giving rise, says Waldeyer, to cancerous formations. This distinction is now disputed. Köster, taking up an idea of V. Recklinghausen, proclaims the exclusive derivation of the cancroid tubules (cancroid zapfen), from lymphatics. In support of his position he calls attention to the striking resemblance of the anastomosing cell-cylinders, best seen in sections made through the peripheral portions of the tumour parallel to the surface, to a lymphatic net-work. Further, he claims, by means of staining the tissues with nitrate of silver, to have established the connexion between the cell-cylinders and the lymphatics, and to have been able to trace the transition from cancer-cells to the delicate epithelium lining those vessels. Köster is probably too exclusive. It is not likely that all observations, previous to his own, have been, as he suggests, faultily made and interpreted; still he may be fairly considered to have opened a new field, worthy of further study. Robin says that, in certain tumours, taking their origin from the serous membrane, the essential elements are really epithelium, and that the differences that they present to that of other epithelial tumours correspond to the normal differences between the epithelial cells of serous membranes and those of the mucous membrane of glands.

"*The Invasion of Neighbouring Organs.*—The further progress of the disease is the necessary consequence of the intimate connexions of the uterus with the contiguous organs. It extends from the cervical mucous

membrane to the muscular structure of the cervix, and upward toward the body of the uterus. The mucous membrane of the vagina becomes covered with secondary granulations. Generally, in *post-mortem* examinations, the limits between cervix and vagina can only be determined by their relations to the bladder and rectum. The cellular tissue around the upper part of the vagina and uterus, and between the folds of the broad ligaments, becomes invaded by glandlike tubes, or isolated granulations. Thence the disease spreads to the bladder and rectum, attacking successively the intermuscular and submucous connective tissue, and finally the mucous membrane. From the bladder it creeps upward to the apertures of the ureters. Sometimes both anterior and posterior *culs-de-sac* of the uterus become filled with cancerous deposits. The pelvic glands are always tumefied, and frequently transformed into a mass like the diseased structure of the primitive tumour. Single glands may attain the size of an apple; a plexus may become the size of the fist, or even larger. Sometimes the ovaries are involved, though it is difficult in necroscopic examinations to determine whether a degenerated tumour glued to the postero-lateral surface of the uterus is an ovary or lymphatic gland. Wagner found the ovaries reported as affected in twenty-two cases out of 171, in most of which the degeneration was propagated from the fundus uteri. From the beginning of the malady, there is marked pelvi-peritonitis, with formation and organization of false membranes. In this way adhesions form between the Fallopian tubes and the adjacent organs, and the whole pelvic contents become at last reduced to a nearly solid immovable mass.

"*Modes of Propagation.*—Virchow has shown that the growth of tumours takes place by the formation of fresh nodules about the periphery, or of detached groups at a certain distance from the original tumour. Now, if we adopt the theory of genesis, the mode of propagation presents no difficulty. Thus Cornil, who has admirably described the glandular origin of cancer of the breast and pylorus, consisting, in the first stages, of hypergenesis of the gland-cells, filling up of the central cavity, and formation of lateral and terminal offshoots, ascribes the peripheral nodules to the consentaneous genesis of new glandular tissue. Now, in this explanation, the difficulty lies purely in the thing itself for which genesis stands. Virchow, who has the merit of showing that the primitive tumour is the starting-point of infection, supposes a special pernicious juice to be generated by the primitive tumour, which extends by imbibition to the surrounding tissues, and awakens in them a series of changes terminating in the formation of neoplasms of the same character. These infectious juices are carried to the glands, and thence disseminated to distant parts by the lymphatics and veins. These juices, which have been compared to the poison of syphilis, are, however, very curious in this respect, that they give rise in the secondary nodules to precisely the same morphological

constituents as those of the primitive tumours. To be sure, we not uncommonly find that the lymphatics proceeding from the tumours are filled for a considerable distance with so-called cancer-cells. Nor is it at all rare for masses of the tumour to penetrate into the calibre of veins, where they form soft vegetations, from which particles can hardly fail to be swept by the current of the circulation. But Virchow supposes that in the dissemination of tumours the formed elements act simply as carriers of infection. Thiersch, however, thinks we can hardly admit that a contagious material would be capable of imparting to cell-proliferation a distinct histological stamp, so that the tissue affected could assume the type of a widely-different order of development. On the contrary, he esteems it much more probable that, among the elements carried into the circulation, there would be some, in the process of multiplying at the moment of being swept away, which would hardly forfeit this faculty by a short transportation in a stream of blood or lymph. In such a case we would naturally find the same forms in the secondary as the primary seat of the disease. Waldeyer describes a mode of epithelial emigration, which must play an important part in the production of secondary formations. At first, there is almost always a sharply-defined limit, composed of short cylindrical cells, about the epithelial products, but after a time the line of demarcation gives way, and narrow, cylindrical, or spindle-shaped passages form, containing long lines of epithelial elements, which can be followed far into the connective tissue, from whose corpuscles they may be easily distinguished. These passages or gaps Waldeyer regards as the starting-points of the lymphatics. Köster denies that these lines of cells are the beginnings of metastases following the lymph passages, but holds them to be themselves altered lymphatics, and to constitute an early stage in the course of cancerous development. If Köster's view be correct, that the epithelial elements of the secondary as well as the primary tumour are derived from the cells of lymphatics, the question whether metastases are due to the washing away of fluid or formed constituents becomes of inferior importance."

MALIGNANT DISEASES OF THE UTERUS—CLINICAL HISTORY AND TREATMENT.

Dr. Fordyce Barker read a very valuable paper on malignant diseases of the uterus before the New York Academy of Medicine on 17th Feb., 1870.[a] The observations are confined to the clinical features of these diseases, and the first point considered is their frequency. Dr. B. shows good reason to believe that the apparent

[a] Some Clinical Observations on the Malignant Diseases of the Uterus. By Fordyce Barker, M.D. Transactions of New York Academy of Medicine. Vol. iii.

increase in their frequency is due to the more perfect means used now for their diagnosis; and he hesitates to accept the assertion that the disease is more frequent in cities than in the country, and in temperate climates than in warm; but he believes that the African race is comparatively exempt from cancer of the uterus.

The hereditary nature of the disease Dr. Barker doubts very much, but he has found it very frequently associated with phthisis, and after reviewing the statistics on the subject, he proceeds as follows:—

"Now, to my mind, all that these statistics prove is, that a vitiated constitution—a diathesis, whether it be inherited or acquired, whether it be tuberculous or cancerous, may predispose to the development of cancer. All the other causes which authors have assigned as predisposing to this disease, amount to just this and no more. I should say, then, nothing is known in regard to the *etiology* of this disease, as science up to the present time utterly fails to explain what are the causes of this special form of heteromorphous development. I shall add, there is no evidence that the *disease* is, in any proper use of the term, *hereditary*. The *diathesis* is, undoubtedly, often inherited. But it does not follow that the child of a parent who has died from cancer necessarily or even generally inherits the diathesis; for both the diathesis and the disease of the parent may have been acquired subsequent to the conception and the birth of the child."

The classification, symptoms, and diagnosis of the disease are next considered; but it is not necessary to dwell on these sections. That on the duration is more important; but it is not possible to avoid having a doubt as to the accuracy of the diagnosis in these very prolonged cases. I have myself seen some very prolonged cases, which in their early stage presented all the features of malignant disease; but the very fact that they did not run the usual course has made me very doubtful as to the correctness of the early diagnosis:—

"*Duration of the Disease.*—The physician in attendance in a case of malignant disease of the uterus is constantly asked how long she may live. Relying upon the statements of authors on this point, my answer formerly was generally wrong, as I found that my patients almost invariably outlived my expectations. Lebert gives the average duration in 39 cases as a little over sixteen months. West, in 17 cases, found the average duration 15 months. Simpson says: 'Patients usually die in from two to two years and a half.' From personal observation I can give the duration from the time of the first symptoms of the disease to

the time of death in only twenty-six cases, fifteen of which have been under my own charge, and the remaining eleven I have seen in consultation with others. The average duration in these 26 cases was three years and eight months. I cannot explain the difference between these results and those of the authors before quoted. I only know the facts to be true. In one case now living, it is eleven years since I first made the diagnosis of cancer of the uterus. She is still in such health that she visits her friends, attends church quite regularly, occasionally goes to the opera, and no one suspects the existence of the disease except her husband, myself, and an old nurse, who has been with her since the birth of her first child, now 26 years of age. This nurse has acquired great dexterity in making applications to correct the fetor and restrain the leucorrhœal and hæmorrhagic discharges. It is at least four years since the cervix was entirely destroyed, and more than a year since, in a vaginal examination, the finger passed into a large cavity, so that the walls of the uterus seem to be a mere shell. I have another patient still in a pretty good general condition, in whom I recognized the existence of cancer nearly five years since. I am glad to observe that Courty in a great measure confirms my experience on this point. He says:—'I have very positively seen women succumb to the progress of uterine cancer several years, sometimes seven or eight, after the probable epoch when it had commenced to develop.'"

On the treatment Dr. Barker speaks more hopefully than most of us would be inclined to do. That cancroid or epithelial cancer may be successfully treated by ablation, if the operation be performed sufficiently early to extirpate the entire diseased structure, cannot now be questioned. Dr. Barker has himself ablated the cervix since 1856 in eleven cases for this cause, nine of which have recovered and remain well; in one death followed in two days from peritonitis, caused by imprudence; in the other the disease recurred in a few months, and the patient died. Dr. Barker refers to Mr. Hutchinson's report of fourteen cases operated on, in which eleven recovered, and proceeds to say:—

"In view of all these facts, it must be conceded that science has made great progress in successfully treating a class of diseases, which until quite recently invariably resulted in a fatal termination. But the same success cannot as yet be claimed as to the other forms of malignant diseases of the uterus. And yet I cannot but feel that there is much to encourage us to hope that in the future we may yet attain a very much greater success in the treatment of this most terrible disease, than as yet has been dreamed of by the most sanguine of scientific men. In the first place, we are making rapid progress in our knowledge of the histology

of the disease, and the laws of its development. In the next place, facts are accumulating which show that spontaneous recovery has occurred in some cases, and art may yet, in a certain per-centage, secure the same result. Aran quotes one case from Estevenet, in which the cancerous uterus was separated entirely from its attachments, and expelled by the woman in the effort of defecation, so that for some time there were hopes of her cure. Prof. Habit reports one case of spontaneous recovery, in which the cancer involved both the uterus and the vagina. Virchow, Kiwisch, Rokitansky, and Scanzoni, each report one case of spontaneous recovery from cancer of the uterus. Two cases of recovery have occurred in my own experience, unless there was some mistake in the diagnosis. As both of these cases were of great interest to me, I shall detain the Academy by a somewhat detailed history of each."

Of these two cases of recovery, one was treated, in 1852, with the actual cautery which was applied some fifteen or sixteen times, and the patient is still alive. There is no description given of the case, but the woman herself stated she had been seen by Mr. Mott and Professor Bedford, and that they told her she had cancer. Dr. Barker himself seems to doubt the diagnosis. "I know not," he says, "whether in the light of my accumulated experience since that time I should regard that case as one of cancer or not, but I think there can be little doubt as to the following:"—

"In 1856, the wife of a prominent lawyer and politician in Massachusetts came to this city for consultation. She had previously been under the care of the late Dr. Green of Worcester, and been examined by one or two eminent physicians in Boston. Her husband informed me that they had called her disease 'cancer of the womb.' She was cachectic and very feeble, and for more than a year she had been suffering from severe and frequently recurring pains, repeated and profuse hæmorrhage, with a constant, sanious, and extremely offensive discharge. The cervix was much enlarged, with a jagged, irregular surface, and the uterus was fixed and immovable in the pelvic cavity. My opinion as to the nature of the case coincided with that which had been expressed by those who had seen her before, and, of course, my prognosis was most unfavourable. But with the hope of diminishing the profuse vaginal discharge and the extremely offensive odour, and that by accomplishing this I should make her journey home more comfortable, I decided to apply the acid nitrate of mercury freely to the ulcerated surface of the cervix.

"Some two weeks afterwards I received a letter of bitter reproach from her husband, in which he said that soon after her return her mouth became very sore, and her physicians declared it to be the worst case of

salivation that they had ever seen. Several of her teeth had come out, and the discharges from the vagina had increased enormously, with which she had thrown off masses of putrid and horribly offensive substances, so that it was difficult to remain in the room with her. The letter closed by saying:—'Before you receive this my dear wife will have left me, and I shall always bear in my heart the feeling that you, by your experiments, have greatly added to her sufferings and shortened her days.' A few months afterwards I received a letter from him of quite a different tenor. She had quite recovered her health, and the disease was apparently cured. I have twice, within a few years, had the opportunity of making a vaginal examination of this lady. At the upper portion of the vagina a puckered cicatricial indentation can be felt and seen by the speculum, but nothing like the cervix or body of the uterus can be felt through the vaginal walls. Through the rectum I am able to detect what I suppose to be the remains of the body of the uterus. She is now 59 years of age, fat and stout and in robust health. It is an old adage that converted enemies become the most zealous of friends."

For constitutional treatment Dr. Barker speaks favourably of arsenic as improving the general condition of the patient, as to flesh and strength, diminishing the discharges, mitigating the pain, and arresting the progress of the disease. He gives Fowler's solution in three drop doses. The Missisquoi water of St. Alban's, Vermont, has in some cases seemed to Dr. Barker of great benefit in arresting for a time the progress of the disease; but in many it seemed to produce no effect.

For local treatment Dr. Barker recommends amputation in all cases where the cancroid disease is confined to the cervix, and can be wholly removed. He always uses the knife for this purpose, and not the écraseur, because he thinks it leaves the surface in a much better condition for healthy cicatrization, and he has never seen any trouble from hæmorrhage. If the disease have extended beyond the reach of the knife he recommends cauterizations; and notwithstanding the objections made to them he believes they are often very useful:—

"The principal agents which I have used for this purpose in the malignant diseases of the uterus are the actual cautery, the acid nitrate of mercury, and the chromic acid. In those cases where the malignant deposit has not extended to the adjacent organs, and particularly to the mucous membrane of the vagina, I use the actual cautery far more frequently than any other agent. My reasons for this preference are the following: It is a much more manageable agent than any other, as it

only acts upon the part with which it immediately comes in contact; the depth of cauterization can be perfectly regulated; no pain is produced by its use, either at the time or subsequently; and when the slough is thrown off no hæmorrhage follows, as I have often seen occur when other agents are used.

"I have chiefly used the acid nitrate of mercury in those cases where granulations sprout from the interior of the cervix, which have a great tendency to bleed. But for some years past I have become very cautious in its use, as in three instances I have caused profuse salivation by one application of this agent, and I have heard from medical friends of several other cases of like result. The susceptibility to this effect seems to be manifestly increased in a broken-down, cachectic system, and in such it is a consequence greatly to be deplored.

"In cauliflower excrescence, where the disease has extended beyond the reach of the knife, I generally make use of the chromic acid (100 grains to the ounce of water), applying it very carefully with a pencil brush to the surface. Ordinarily its action is not followed by pain, but I should mention that in one case, six hours after the application of the chromic acid, severe pain came on, so that I was sent for in the night, and, after using three hypodermic injections of morphia without relief, I was obliged to keep my patient under the full anæsthetic influence of chloroform for nearly four hours. A pretty severe attack of peritonitis followed, from which she entirely recovered in a few days. There was complete and apparently healthy cicatrization of the diseased surface, and for more than a year I deluded myself with the hope that the disease was cured. But when it again appeared its progress was very rapid, and she died in eight weeks after the first symptom of its occurrence. I have repeatedly made trial of various other caustics, such as the arsenical paste, chloride of zinc, etc., but without such results as lead me to substitute either of them for those above mentioned."

For palliative treatment he gives sedatives, and he expresses a very decided opinion, that we are bound to give our patients relief from pain if we can, no matter what the agent is or how large the quantity or how long it may be continued. It has been said that a too early use of opium may lead to a habit which will injure the general health; but protracted pain, Dr. Barker urges, will injure the general health much more than any agent which may be necessary for its relief:—

"In some cases, there is great suffering from the extreme sensitiveness of the ulcerated surface, and this I have found best relieved by pessaries of iodoform, introduced at night into the vagina.

℞

Iodoform,	gr. x.
Butyri cacao,	ʒi.
Glycerine,	gtts. 5.

M. One pessary.

"These pessaries seem also to correct almost entirely the offensive odour of the discharges.

"As lotions for arresting the discharges and removing the fœtid odour, I have found the most useful to be a solution of bromine, or of permanganate of potash, or of carbolic acid. Where the cervix or any portion of the vagina is ulcerated, I am careful to direct that these lotions should not be forcibly projected against the diseased surface by any form of syringe, but that the vagina be filled by a syphon-tube from a vessel placed considerably above the patient. The 'fountain syringe' accomplishes this result in a very convenient manner."

Dr. Routh[a] describes two cases of epithelial cancer treated by local applications of bromine. In the first there was a mass of fungoid epithelial growths, taking their origin from the os, and about the size of an egg. The granulations were broken down, and the actual cautery applied to stop the bleeding. When the slough came away the bromine was applied. A solution was made by adding five minims of the pure bromine to fifty of spirits of wine. A piece of lint, the anterior surface of which was well saturated with this solution, was applied to the uterine diseased surface, and kept *in situ* by pledgets of lint. This was left on forty-eight hours. A thick ashy slough formed and separated, leaving a large cavity with granulations, which looked healthy, but bled on the slightest touch. To this a saturated solution of tannin in glycerine was applied on lint, and repeated every day. The wound healed "kindly," except in two places, and Dr. Routh applied the bromine again, but used a solution of ten drops in fifty of spirit. He placed the lint in a glass cup, which he kept in its place by pledgets of cotton dipped in a saturated solution of carbonate of soda. The slough separated in a week; and the part healed and seemed to be healthy; but it was now found that the interior of the uterus was affected with the disease, and another application of the bromine was made, and after some time "she left hospital with a movable healthy uterus, the os and lining membrane covered with healthy mucous

[a] On a new mode of treating Epithelial Cancer of the Cervix Uteri and its Cavity. By C. H. F. Routh. Transactions of the Obstetrical Society of London, Vol. viii., p. 290.

membrane; she herself fat and hearty, and declaring she had not felt so well for years."

In the second case there was a large carcinomatous mass, about the size of a large orange, attached to the os. It appeared to be a large cauliflower excrescence, breaking down readily at the slightest touch, with carcinomatous odour of an unusually offensive character. This was removed with an écraseur. A few days afterwards the bromine was applied, and afterwards, as it was found the disease extended into the interior of the uterus, the bromine was applied there also. "She left the hospital with no traces of the disease; movable uterus covered with healthy mucous membrane; looking fat and hearty, and fully twenty years younger than when she came in." Dr. Routh believes the bromine seems not only to arrest the disease locally, but also the cachexia which accompanies it. The depth to which the caustic pervades the cancerous growths, while the vitality of the unaffected parts seems stimulated to increased reparative activity, he also thinks remarkable.

Dr. Routh gives a necessary caution as to the use of the bromine; for not only is it not unattended with danger to the patient, but it also exposes the operator to great risk, for it emits very acrid fumes, and is liable, if not mixed very gradually with the spirit, to explode. The solution is very volatile, even when carefully mixed, and should be kept in glass-stoppered bottles, or, better still, prepared in another room just before it is used. This volatility, Dr. Routh thinks, enables it to permeate better the diseased parts; but as it may also affect the vagina backwards, he looks on the cup in which it is placed, and the cotton dipped in the carbonated alkalie, as very necessary adjuncts in its application. Dr. Routh thinks arsenic, given internally, a useful concomitant, and believes it does good in some cancerous cases, and checks their progress. He thinks the solid form is the best way of giving it, as in this way it is assimilated as it descends the intestinal tract, and does not produce the gastric and ocular annoyances which the liquid form does, and so larger doses may be taken. It was given in both of the cases related in this paper.

Dr. A. Wynn Williams records seven cases of cancer treated by bromine.[a]

[a] Cases of Cancer of the Womb successfully treated by Bromine. By A. W. Williams, M.D. Transactions of the London Obstetrical Society, Vol. xii., p. 249.

In the more superficial, as some of the epithelial forms, the bromine was applied in the manner recommended by Dr. Routh, on lint placed in a glass or vulcanite cup; but for solid growths, Dr. Williams uses a fine trocar and canula and injects the bromine into the substance of the growth, and in the after treatment he attaches great importance to the use of vaginal injections of a weak solution of bromine.

The same criticism applies to Dr. Williams' cases as to Dr. Routh's—that if the disease be epithelioma or cauliflower excrescence, ablation or amputation of the cervix, where it can be performed, is not only a safer, but a more efficacious and less painful mode of treatment, and if the disease be the true medullary carcinoma the bromine will probably not be more useful than strong nitric acid, the application generally adopted in the Dublin hospitals. During the discussion which followed the reading of Dr. Williams' paper it was very properly observed that there was no proof afforded by the description of the cases, that they were examples of malignant diseases at all, and it is to be feared that neither Dr. Barker, Dr. Routh, nor Dr. Williams has discovered any cure for cancer of the uterus of greater value than those that have been hitherto in use.

II. RETRO-UTERINE HÆMATOCELE.[a]

Dr. Barnes contributes, under this heading, the histories of fifty-three cases, divided into six groups, and the last group subdivided into five sub-groups. The paper opens with a eulogy on the uterine sound and speculum, which the author says were used by Harvey; and it would not be difficult, according to Dr. Barnes, to prove that Harvey's knowledge of uterine pathology was more extensive and accurate than that possessed by many physicians of the present day, who deny themselves, and would deny to others, the aid of those means which he knew to be essential. Had these means been employed, Dr. Barnes says, by Harvey's successors, it is scarcely possible that a discovery so important as the pathology of pelvic hæmatocele would have been postponed until the middle of the nineteenth century. Let this be as it may, there can be no doubt we are indebted to Bernutz and Goupil, and to Nélaton and other French pathologists for having

[a] Cases illustrating the Clinical History and Pathology of Effusion of Blood into the Peritoneum, with special reference to the so-called Retro-Uterine Hæmatocele. By Robert Barnes, M.D., St. Thomas' Hospital Reports, New Series, Vol. i. 1870.

first directed our attention to this subject. Dr. Barnes limits the scope of his paper to effusions of blood *into* the peritoneal cavity; cases of *extra* peritoneal effusion, though allied, demand special study. He gives the following outline of the symptoms and diagnosis, from which it will be seen that the real difficulty is to distinguish the majority of such cases from peri-uterine phlegmon; but this is the less important, as when the difficulty arises the treatment is the same :—

"The common symptoms of the 'intra-peritoneal hæmatocele' are those which mark all sudden injuries attended with effusion of blood within the abdomen. These are, primarily, the signs of shock, viz., pain, prostration, collapse more or less complete, cold surface, pallor, perhaps syncope, generally vomiting. Secondarily, there are the consequences of peritoneal irritation; pain, increased on pressure, distention of the abdomen, quickened pulse, increased temperature, aspect of suffering. The blood naturally gravitating to the retro-uterine pouch of the peritoneum, will, in the less severe order of cases, generally clot there, and form with the superadded peritonitic effusion which surrounds it, encysting it, a mass more or less firm and rounded, which pushes the uterus and the roof of the vagina bodily forwards against the symphysis pubis, and compresses the rectum backwards. The pressure thus occasioned, just like that from retroversion of the gravid womb, may cause retention of urine, and commonly causes tenesmus of the bowel. That the tumour occupying the pelvis is really outside the uterus can be readily demonstrated. The finger passed into the vagina is directed by the tumour forwards and upwards to the os uteri, close behind the symphysis pubis. Behind the os, and stretching below it, is the resisting rounded tumour bulging forward the roof and posterior wall of the vagina. The finger passed into the rectum gets below and behind the tumour, but can rarely reach above it; keeping this finger on the tumour, whilst the fingers of the other hand are pressed into the abdominal wall above the pubes, the tumour is defined between the two hands; perhaps some obscure fluctuation is felt; if the mass be small, its situation will be found limited to the pelvis; but if large, it may be felt rising to any point between the symphysis as high as, or even above, the umbilicus. Generally the fundus uteri, which is pushed closely against the abdominal wall, may be felt as a firm mass rising just above the symphysis, and forming a distinct projection, separated by a groove from the blood-tumour behind and above it. The uterine sound completes the diagnosis. The instrument, slightly curved, is passed into the cervix uteri, and the point turned forwards towards the umbilicus, proceeds until it is arrested by reaching the fundus of the organ. Then nothing is more conclusive than the evidence it gives. It has passed two and a

half inches—the normal length of the uterus—or more, showing that it really defines the uterus; and by carrying the handle gently backwards, so as to make the fundus project over the symphysis, the hand plainly traces the outline of the uterus there, and it may even feel the point of the sound. The position of the uterus thus being determined, it follows that the tumour behind is something else. This cannot be done in retroversion of the uterus. If it be possible to pass the sound, it can only be by giving it a strong curve and turning the point well backwards after entering the cervix. A skilful hand will not be likely to mistake the tumour for a fibroid.

"Peri-uterine phlegmon is distinguished by the local symptoms coming on after labour or abortion, by the symptoms being less marked and severe at the onset; by its course being slower; by the chief seat of the tumefaction being lateral, not retro-uterine; by the os uteri being found fixed nearer the centre of the pelvis, not being driven forwards. The patient commonly walks lame, dragging one leg."

The first and second groups consist of cases where the hæmorrhage depends on rupture of the uterus or rupture of extra uterine-gestation cysts; but the propriety of classing these cases along with the other groups is very doubtful—doing so seems to unnecessarily complicate the inquiry. The third group embraces four cases, in which the hæmorrhage arose from the rupture of diseased ovaries. In two the ovary was diseased giving rise to rupture, from which the hæmorrhage proceeded. In one there was a simultaneous advance of pregnancy and growth of an ovarian cyst, with axial twisting and rupture of the cyst; and in another there was the same axial twisting of an ovarian cyst, but without the pregnancy.

Group IV. has only two examples. The hæmorrhage arose from injury. In one case a woman carrying a child, in great haste to catch a train, felt something snap. It was suspected she was pregnant at the time. She got pain in the pelvis, which continued three weeks, with some sanguinous discharge and febrile movement. Dr. Barnes found pain and swelling behind Poupart's ligament in the left side. The uterus was rather low in the pelvis; the cervix was directed to the right. In the seat of the left broad ligament was a firm deposit, fixing the uterus and pushing it to the right. She was recommended rest, and recovered.

Group V. consists of cases of effusion of blood into the peritoneum attending abortion. Several of these cases seem to be examples of pelvic cellulitis, though the hæmorrhagic nature of some is proved by the escape of large clots on the opening

of the tumour. During abortion, if there should be any obstruction to the escape of the blood from the os uteri, it seems to Dr. Barnes not improbable that under the extreme tension of the vessels from their increased turgescence, escape may take place by the Fallopian tubes into the peritoneal cavity. Eight cases of effusion attending abortion are given, but it does not appear at all evident that any of these arose from an obstruction to the escape of blood from the os. It is the more important to note this, as, if it were otherwise, we might hesitate to adopt plugging in cases of abortion, and thus expose our patients to the risk of very serious hæmorrhage. As the next group is the most important, and includes the cases that are most correctly termed peri-uterine or pelvic hæmatocele, it may be well to give in full Dr. Barnes' introductory remarks:—

"Group VI.—Menstrual Disturbance or Difficulty, leading to Effusions of Blood into the Peritoneum.

"This group includes by far the largest proportion of cases. At the same time the danger is considerably less, and the symptoms are less severe. It may be stated as a general rule, that, whenever there is any impediment to the free discharge of the menstrual blood by the natural route, if the quantity of blood exuded in the uterine cavity be excessive, or suddenly increased by accident, by emotion, or other causes, escape may take place by the Fallopian tubes into the peritoneum.

"It will be instructive to subdivide this group under heads referring to the presumed exciting or chief cause of the blood effusion. We thus get sub-groups containing.

"1. Cases of probable very early Fallopian gestation and escape of ovum into the peritoneum.

"2. Cases in which there existed a mechanical impediment to the natural escape of the menstrual blood.

"3. Cases in which there was disturbance or interruption of the menstrual flow from

"*a.* Cold and over-exertion;

"*b.* From emotion.

"4. Cases in which the immediate cause was obscure, the history in some being imperfect.

"5. Cases in which the hæmorrhagic character of the blood was increased by disease."

In the second of these sub-sections Dr. Barnes gives two cases, caused by menstrual retention at the onset of the function. The uterus, comparatively immature and unused to the duty it is called

upon to perform, does not, he thinks, expand to accommodate the blood poured into its cavity, and which is retained by an imperfect development of the cervix from being discharged by the natural outlet. There is consequently reflux along the Fallopian tubes, hæmatocele, and peritonitis. A very similar explanation will apply to the remaining cases in this sub-section where the menstrual difficulty arose from contracted os and cervix uteri; but the mechanism by which hæmatocele is produced in the cases included in the remaining subsections is not so evident, especially in those in which menstruation is disturbed by cold and over-exertion, or by emotion; and yet these cases are not only the most frequent but the most important, especially in a diagnostic point of view.

In conclusion, Dr. Barnes reviews his cases, and points out, first, the difference between encysted and non-encysted effusions. The first great fact, he says, that strikes the observer is that when the effused blood does not become encysted, death results; whereas, when it does become encysted, recovery is the rule. It does not, indeed, follow that the presence of pure blood in the peritoneum is the cause of death, although it is certain that its isolation is a curative process. The failure of the encysting process may depend on the crushing severity of the primary lesion and the large quantity of blood poured out. The shock and anæmia may be so great that the patient is killed in perhaps a few hours; or even if the patient survive a few days, the blood remaining in the vessels may be deprived by the shock of its natural power to exude plastic matter.

The encystment of the blood is the first step towards getting rid of it. It may be discharged in mass by an opening in the vagina or rectum, or it may be softened and absorbed, or evacuated slowly by a small opening into the vagina or rectum. It has been suggested that it may sometimes pass along the Fallopian tubes, and so escape by the uterus; but Dr. Barnes does not think this correct, but let it be as it may, he believes, in opposition to Courty, in the possibility of the complete disappearance of the hæmatocele.

The source of the blood in cases of rupture of the uterus, tubes, or ovaries, is clear enough; but in cases of abortion or menstrual difficulty it may be questioned whether it come from the Fallopian tubes, from the ovaries, or varices of the broad ligaments. Virchow affirms it comes from the new-formed vessels of inflamed peritoneal surface; but Dr. Barnes concludes, from his own observations, that the peritonitic source must be extremely rare,

and that the general opinion that declares the peritonitis to be secondary, not primary, is correct. A most careful consideration of the greater number of cases where hæmatocele ensued on abortion or disturbance of menstruation, leads Dr. Barnes to concur with Trousseau, who contends that the blood comes from the Fallopian tubes, copious exudation of blood from the mucous membrane of the uterus and tubes as an antecedent condition to intra-peritoneal effusion is indeed one of the most constant facts in the history of this affection. When this outpouring of blood is in excess of what can be readily discharged by the uterus and vagina, it is easy to understand that some may be driven back by the Fallopian tubes. The retrograde escape of menstrual dysmenorrhœa in small quantities is not very uncommon. The agonizing pain, quickened pulse, hot skin, following upon signs of shock not unfrequently observed in this form of dysmenorrhœa seem to point to this conclusion. Trousseau relates a case in which extreme retroflexion prevented the escape of menstrual fluid through the os, and caused it to make its way along the tubes and form an intra-peritoneal hæmatocele. The possibility of this dangerous issue of obstructive dysmenorrhœa is a reason to be added to others for curing the obstruction.

For the treatment Dr. Barnes recommends, in the first place, rest and opium, with ice to allay vomiting. If the uterus or an ovarian cyst be ruptured, gastrotomy should be the rule, and also, perhaps, in cases of tubal gestation, especially if three months of pregnancy have passed. In abortion cases and obstructive dysmenorrhœa, care must be taken to secure free dilatation of the cervix uteri; and as soon as possible to remove the ovum and membranes. The laminaria tent should be used if the os internum be very small. When effusion has taken place four indications arise—1st, to support the patient under shock; 2nd, to restrain further hæmorrhage; 3rd, to moderate inflammation; 4th, to favour the removal of the hæmatocele. For the first and second indications little need be said. For the third Dr. Barnes recommends rest and opium. Leeching from the groin, poultices, and mercury in moderate doses. As to the fourth indication, the important question is as to whether the hæmatocele should be opened. Opinion, he says, seems generally to be settling in favour of non-interference; and his own observation tends to confirm the soundness of the conclusion. He is disposed to limit resort to puncture to cases where there is evidence of blood infection from suppurative action.

HALF-YEARLY REPORT ON PUBLIC HEALTH.[a]

By Charles A. Cameron, Ph.D., M.D., L.K. & Q.C.P.; Prof. of Hygiene in the Royal College of Surgeons; Lecturer on Chemistry in Steevens' Hospital Medical College, and in the Ledwich School of Medicine; Analyst to the City of Dublin, &c.

MILK AS A CARRIER OF DISEASE.

There is evidence to prove that the milk of the lower animals is capable of transmitting disease from the animal producing the liquid to the animals—including man—who drink it. It is bad enough that milk—the papulum of so large a proportion of our population—should occasionally afflict us with foot and mouth disease, derived in a direct manner from the cow; but what must not be our alarm when we are positively assured that a draught of milk may possibly introduce into our bodies the virus of typhoid fever! Dr. Ballard, Medical Officer of Health for Islington, London, has investigated into the cause of a local outbreak of typhoid fever in his district, and to our mind clearly traced it to the use by the affected persons of milk supplied from a particular dairy. Between the 3rd of July and the 10th of Sept., 1870, 168 persons, part occupants of 67 houses in the district of Islington, contracted typhoid fever, and in 26 of the cases the disease terminated fatally. With but few exceptions the persons attacked were in comfortable or affluent circumstances, and were attended by private practitioners. The radius of the district in which the 168 cases of typhoid fever occurred is a quarter of a mile. Outside this radius the number of fatal cases of typhoid fever which occurred in the whole of Islington, which has an area of 3,127 acres, amounted to 20. Six of these cases, Dr. Ballard ascertained, had originated outside the parish of Islington, leaving the source of 14 to be determined. One of the 26 persons who died within the limited circle contracted the disease in the country.

As the people in the limited circle were living under very fair sanitary conditions, in good houses and in a well sewered locality, Dr. Ballard came to the conclusion that all or nearly all the cases were due to a common and strictly local cause, which he proceeded

[a] The author of this report will be glad to receive any books, pamphlets, or papers relating to hygiene, dietetics, &c. They may be forwarded through the agencies of this Journal.

to investigate in a thoroughly exhaustive, careful, sagacious, and scientific manner. He found that all of the families amongst whom death from typhoid fever had occurred were supplied with milk from the one dairy. He ascertained that the owner of this dairy had died from typhoid, and that seven of his family and servants had suffered from the disease. It appeared also that typhoid had occurred in a family who lived in a small cottage in the cow yard belonging to the milk vendor, and situated about 100 yards from the dairy. Several other persons employed about the dairy, but who did not reside upon the premises, were likewise affected with typhoid. Thus we find there was abundance of typhoid poison generated at this dairy.

It appears that 142 families were supplied with milk from this dairy The district in which those families resided contained a population estimated at about 2,000 families, so that only a small proportion of the inhabitants of the district was supplied with milk from the dairy in question. Out of those 142 families (which included the dairyman's household) 70 were attacked with typhoid. The number of individual cases amounted to 175, of which 30 terminated fatally. "Inquiring," says Dr. Ballard, "as to the source of milk supply in all other instances of typhoid in the parish, during the same period, that came to my knowledge, I scarcely ever heard the name of any one milkman mentioned twice." It is important to notice that the houses supplied with milk from this dairy were not confined to a few streets—they were distributed throughout the whole of the circle above described.

The fact of the distribution of typhoid poison from this dairy having been clearly established, the next question was by what medium was the contagium conveyed? The carriers of the milk certainly did not bring the poison in their clothes, and clearly the contagium must have been conveyed in the milk itself. How did the contagium get into the milk? Dr. Ballard encountered most difficulty in attempting to solve this one of the many problems involved in his inquiry. He says—"It is clear that it," the contagium, "must have got into it (the milk) either before it was sent out from the dairy or subsequently. The only probable way in which it could have got into the milk in its passage from the dairy to the customers is by the addition of foul water by the milk-carriers, in pursuance of a fraud upon their master. But against this supposition we have the fact that the young man who managed the dairy business—the master, indeed (who was one of

the first fatally attacked)—himself carried out a great part of the milk in a cart, and also the fact that the disease, even in the first week of the outbreak, was so widely sown as to preclude the idea of such a fraud having been perpetrated unless by general concert between the master and the men. Neither will it serve to explain the fact that the family of the dairyman was itself attacked very early in the outbreak. The milk that they got is not to be supposed thus fraudulently contaminated by a milk-carrier outside the premises. But besides sending out the milk furnished by his own cows, this dairyman often bought milk for sale from other sources. Sometimes, when the demand was greater than his supply, or when two or three of his cows ran dry, he bought considerably. Was this milk that he bought and served out to his customers, together with his own, that which contained the contagium? I inquired where the milk, under such circumstances, was obtained, and the answer was, from neighbouring cowkeepers, and several that I know were mentioned to me. This was quite sufficient to clear the purchased milk from suspicion; because, if that supplied from any one of these dairies had been the dangerous milk, I should have found the principal cases of typhoid in families supplied by that dairy. Now, as a matter of fact, in my inquiries throughout the parish into cases occurring during the whole ten weeks, I scarcely heard the name of any other dairyman mentioned twice."

Dr. Ballard next describes the dairy premises, and here he found two possible sources of typhoid poison, namely, the offensive effluvia from stable drains, and an underground water tank which supplied a pump. Dr. Ballard had a specimen of the water found, at the time of his visit, in the tank analysed, but the results showed that its composition was similar to that of the "New River" water, which is supplied to that part of London, and with which the tank was in communication. This was discouraging, but Dr. Ballard persevered; the tank was thoroughly explored, and it was discovered that the rats had burrowed into the bottom of the tank, and had established a communication between it and two old drains. Although there was a new drain intended for the conveyance of effete matters from the house, it was seen that during heavy rains, stoppage of the new sewer, or, from other causes, foul liquids and offensive effluvia could readily pass into the old drains, and from thence into the tank. There is no positive evidence that the milk sent out from this dairy was adulterated with water, but

the universal practice of milk adulteration which prevails in these countries leads us to infer that in the dairy in question the milk was mixed with water. There is evidence to prove that the milk was frequently complained of, both on the ground of its being poor and by reason of its offensive odour. It is admitted that two qualities of milk were sold, one at 4d., the other at 5d. per quart. The workpeople also stated that although they did not mix the water from the underground tank with the milk, they used that water for the purpose of washing and rinsing the cans.

Dr. Ballard has undoubtedly proved that typhoid fever was largely spread through the medium of milk, and it is impossible not to believe with him that the contagium was introduced into the milk by water which had been contaminated with sewage. Dr. Ballard was not, indeed, the first to observe this method of disease communication; for Dr. Bell, of St. Andrews, had previously shown that the poison of scarlet fever had been conveyed through the same medium. The cows in this case had been milked by convalescents from scarlatina.

In January, 1870, a woman applied for medical relief at Steevens' Hospital. She was in her usual health when she took a drink of milk which had just been brought from a neighbouring dairy. Instantly she became unwell, with severe diarrhœa and retching. A portion of the milk was examined by us, and we found it to contain 35 per cent. of added water, and to possess a strong odour of sulphuretted hydrogen. On boiling the milk the fœtid odour disappeared. In this case it was clear that foul water had been used to adulterate the milk.

During the last outbreak of cholera in Dublin, Dr. Robert M'Donnell directed attention to an outbreak of the disease amongst some of the prisoners subjected to solitary confinement at Mountjoy Prison. It was considered at the time remarkable that the disease should have attacked persons who were apparently peculiarly protected from contagion, whilst, we believe, not one of the prison officials contracted the disease. The fact was altogether at variance with the current theories of the propagation of cholera. We suggested to Dr. M'Donnell that it was probable the contagium of the disease had been conveyed to the prisoners through the medium of milk adulterated with contaminated water. There was cholera in the locality from whence the milk was obtained, and if we remember aright one of the contractors who supplied the article died from the disease. It is notorious that the milk supplied to

the public institutions of Dublin was at that time almost invariably adulterated with from 16 to 40 per cent. of water; and we are aware that pump, and not pipe, water was and is largely used to adulterate milk in this city.

WATER AS A CARRIER OF DISEASE.

The spread of contagious diseases by means of potable water is not now as it formerly was, a *questio vexata*. It still, however, remains to be ascertained whether or not it is a common vehicle of contagion. Two contagious diseases appear to be the most frequently propagated through this medium, viz., Asiatic cholera and typhoid fever. We have recently examined a large number of specimens of water, which we have strong grounds for supposing, or rather for believing, had been the means of spreading typhoid fever. In two of the departments of the Board of National Education where typhoid fever had broken out rather extensively, we found the pump water—which it appears had been chiefly used instead of pipe water—contaminated with sewage. A large family occupying a house in Kingstown was recently almost extirpated by typhoid. Dr. W. B. Richardson sent us a specimen of the water used by this family (who were, he informed us, great water drinkers), and we found it loaded with organic matter, nitrates, nitrites, ammonia, and common salt, all evidences of sewage contamination. We subsequently learned that the soil pipe of the water-closet delivered its contents into the ground within three feet of the shaft of the pump. A Mr. Rickard brought us last month a jar of water for examination. He stated that he had taken it from the pump of a house at Rathmines in which he had lately resided, and in which he alleged all the members of his family had rapidly lost their health, and one of them her life, from, as he believed, the use of the pump water. He stated that he and all his family were in perfect health when they went into the house in question, but that immediately afterwards every one of them was stricken with fever, or continued diarrhœa. He himself had lost three stones of his weight, and from being a very powerful man had become almost as weak as an infant. On removing to another house an immediate improvement in the health of the survivors of his family took place. The water was almost the worst which has been submitted to us for analysis. It contained 45·02 grains of solid matter in one imperial gallon (70,000 grains), and of this amount organic matter constituted 21 grains. Nitrates, nitrites and ammonia were present,

but not in large quantities, as the organic matter had not undergone much oxidation.

In a large County Infirmary the quality of the water was much complained of. We found on analysing the liquid that it contained 5·8 grains of organic matter, and 12·63 grains of nitrates per gallon. We stated in our report that it was almost certain that direct communication existed between the pump shaft and a sewer. We have just learned that the sewer has been opened, and a leakage into the well detected. These are but a few of the cases of sewage contamination of potable water, and of disease induced thereby, which have within the present year come under our own observation.

In the "Second Annual Report of the State Board of Health of Massachusetts, January, 1871," the subject of the causes of typhoid fever is treated at considerable length. Inquiries were made from medical men in 163 towns of the State, and their replies occupy 60 pages of a large volume. The following is a tabulated statement of the leading queries and their answers:—

1. Have you observed a difference in the prevalence of this disease between houses supplied with water from wells about the premises, and houses supplied with water conveyed from springs or from ponds of unquestionable purity?

Replies.

Yes,	23
No difference has been remarked,	71
Whole supply of town from wells,	18
Indefinite,	51

2. Can you inform us whether, at times when typhoid prevailed, the water of the wells was rising or falling, and whether it was higher or lower than the average for the year?

(If your attention has not been given to the height of subsoil water as marked by the wells, will you have the kindness to note it in future epidemics, and let us know the result?)

Replies.

Rising after being very low,	11
Falling,	16
Very low,	36
Have not observed,	100

3. Have you observed any connexion between typhoid fever and foul

soil, whether from privies, pigsties, manure heaps, or similar collections of decomposing matter lying on the ground?

	Replies.
Yes,	79
No,	45
Doubtful,	39

4. Have you observed any connexion between typhoid fever and putrid air, whether from rotting vegetables in cellars, bad drains, unventilated living or sleeping rooms, or from any other cause?

	Replies.
Yes,	90
No,	36
Doubtful,	37

Ten towns report that typhoid fever is a disease almost unknown among them, and for this reason they can give no information.

The Mortality from Typhoid Fever in the Urban Population of Massachusetts during the last Ten Years.

	Population, 1865 (All Ages)	Total Deaths from Typhoid in Ten Years	Average No. of persons living each year to one Death	Average No. of Deaths each year to 1,000 Persons living
One hundred and forty-seven (147) cities and towns of more than 2,000 inhabitants, - - -	1,044,294	7,888	1,323·90	0·755
One hundred and eighty-four (184) towns of less than 2,000 inhabitants, -	213,468	2,539	840·75	1·189

It appears that typhoid fever is more prevalent in the small towns in the rural districts than in the densely populated centres of manufacturing industries, although the death-rate from all causes is higher in the latter. This fact bears out to some extent the statement lately made by Dr. Stark, of Edinburgh, that the zymotics are, relatively to the number of deaths, as destructive in the country as in towns. They do not, however, sustain the theory put forth by Dr. Stark that the extirpation of zymotic diseases is beyond the power of man. It is shown in the Reports upon this

subject presented to the Massachusetts Board of Health, that the common proximate cause of typhoid fever in that State is organized matter (chiefly vegetable) in a state of decomposition. "Whether the vehicle be drinking water made foul by human excrement, sink drains, or soiled clothing; or air made foul in enclosed places by drains, decaying vegetables, or fish (Swampscott), or old timber (Tisbury), or in open places by pigsties, drained ponds or reservoirs, stagnant water, accumulations of filth of every sort, the one thing present in all these circumstances is *decomposition*."

If decomposing organic matter be then the proximate cause of typhoid, sanitary measures might be rendered as efficacious in the abatement of this disease in villages and the open country as in the most densely crowded towns.

In Great Britain the great majority of sanitarians appear to regard polluted water as a very common, if not the general proximate, cause of typhoid fever. Professor Max Pettenkofer, of Munich, and his numerous followers, believe that impure potable water has but little to do with the spread of the malady. They contend that it is the foul air issuing from the soil, and not from the water, that conveys the typhoid contagium. No outbreak of the disease can occur until the soil is typhoid ripe. The most dangerous condition of the soil is when the ground water is sinking after a previous rise. In Massachusetts it would appear that the period during which typhoid fever is prevalent coincides with that in which the water is lowest in the wells. The editor of the reports on typhoid fever in the Report of the Board of Health suggests that the comparative immunity from this disease enjoyed by the denizens of the larger cities may be due to the pavement and other impervious layers which cover the soil. If emanations from the soil be the usual source of the disease, the safest plan is to sleep in the highest stories of our houses. As for cellars and kitchens, the authorities should prohibit their use as sleeping apartments.

WATER AS A TOXIC AGENT.

The water lately supplied to many of the larger cities in the United Kingdom is remarkably soft, containing only from 1 to 6 grains of mineral matter per gallon of 70,000 grains. Soft water has a sensible effect upon lead; and many cases of saturnine poisoning from soft water which had been stored in lead cisterns, or conveyed through long tubes of that metal, are on record.

The new water supply of Dublin is very soft. The addition of solution of ammonic oxalate—the most sensitive test for lime—to the Vartry water causes either no change or else the very faintest opalescence in the liquid. This water readily attacks soft lead; but we find that an alloy of 96·5 parts of lead and 3·5 parts of tin is not sensibly affected by it. The water pipes (except the iron mains) in use in Dublin are all made of this alloy, the use of soft lead for water pipes being prohibited.

In the Second Annual Report of the Massachusetts Board of Health there is a paper on the subject of poisoning by water containing minute traces of lead. The following query was addressed to a number of persons in different towns:—

"Have any cases of lead colic or lead paralysis occurred in your town or district, in which you have been able to trace the origin of the disease to water-pipes?"

"The replies are from one hundred and seventy correspondents, in as many different places, and are as follows:

Yes, - - - -	41
No, - - - -	101
Doubt expressed, - - -	20
No lead used in the town, - -	8

"It is stated that in certain towns lead pipe is only used to convey water from springs, and that, when allowed to flow continually without plugs or stop-cocks, no harm has been known to follow.

"The negative replies are very brief, and may be construed as meaning generally that no bad effects have been observed, rather than that, in the opinion of the writers, none may occur from the contact of drinking water and metallic lead.

"The affirmative replies are direct and positive, and are usually accompanied by evidence. They occasionally refer to other accidental modes of poisoning by lead, as by hair-dyes, which are almost universally composed of acetate of lead and sulphur in various proportions; also by cider and vinegar drawn through lead faucets."

The amount of lead found in some of the Massachusetts waters varied from about one-eighth of a grain per English gallon to a much smaller proportion. In the water at Claremont, which proved poisonous to the family of Louis Philippe, the amount of lead was seven-tenths of a grain per gallon. According to Dr. Angus Smith, the one-fortieth of a grain per gallon may affect some

people of more than ordinary susceptibility to lead poisoning. Dr. Parkes considers that any quantity in excess of one-twentieth of a grain per gallon may produce bad effects.

In a most interesting and practical little work[a] on Water Analysis by Messrs. Wanklyn and Chapman, the authors consider the question of the poisonous influence which minute quantities of certain heavy metals dissolved in water exercise on man. They estimate the quantity of water consumed by an individual during sixty years to be about 30,000 liters (a liter is little more than $1\frac{3}{4}$ pints). When, therefore, a poisonous dose of lead or other metal is not contained in 30,000 liters of water, the liquid cannot possibly be bad, metallically considered, even if all the metal introduced by it into the body of a man remained therein whilst the water evaporated from it. Water which does not contain so much as $\frac{1}{2000}$ milligram of lead (a milligram is about the 75th part of a grain) is undoubtedly non-poisonous, but the quantity of lead greater than that proportion which may produce toxic effects has yet to be accurately determined. Messrs. Wanklyn and Chapman "suspect" that the minute quantity of $\frac{1}{100}$ milligram of poisonous metal per liter may produce effects upon the health of those who constantly drink such water. Very much larger quantities of lead exist in cistern water all over this country; and it may yet be found that this metallic impregnation of a liquid which must be daily consumed is a more frequent cause of disease than we have hitherto been led to believe. Several serious cases of poisoning produced by water impregnated by lead have come under our observation.

PROPAGATION OF CHOLERA.

In 1868 the Government of India appointed Dr. A. C. C. De Renzy Sanitary Commissioner for the Punjab. He has published two voluminous reports,[b] one for 1868, the other for 1869. Both volumes contain an immense amount of valuable information relative to the sanitary state of the Punjab, and in relation to sanitation generally in India. Every army medical officer who is serving or about to serve in India should provide himself with copies of these important reports.

[a] Water Analysis. A Practical Treatise on the Examination of Water Analysis. By J. A. Wanklyn, M.R.C.S., and E. T. Chapman. Second edition. London: rübner and Co. 1870.

[b] Report on the Sanitary Administration of the Punjab, 1868. Lawrence Press, Lahore, 1868; also for 1869.

Dr. De Renzy minutely examines the theory of cholera propagation proposed by Pettenkofer (which has been fully explained in a former report on Public Health in this Journal), but concludes that it "gives no assistance in interpreting the phenomena of cholera in the Punjab." Dr. De Renzy also dissents from Dr. Bryden's theory, which has to a great extent been accepted and acted upon by the Indian Government. This theory is as follows:—

"That the cholera virus is generated in the soil of certain districts in the Lower Provinces, where it has a permanent perennial existence; that from time to time it is thence carried by moist air as its vehicle to the Upper Provinces, where it has no permanent existence, but dies out after a period of usually four years, to be renewed after an interval by invasion from the Lower Provinces; that during its vital period it becomes dormant for a certain interval each year; that by observation of the natural laws which control it, we are able to define to a day when a dormant or latent cholera shall reappear; that certain areas are occupied by cholera in each year, while others are free from its presence; that thus parallels are formed, from the study of which we can tell before-hand what will be the geographical distribution of an epidemic; that as these parallels occur in complete subordination to meteorological influences, epidemic cholera is never in any case spread over a definite geographical area by human intercourse, nor can human agency cause the boundaries of a natural province which has been occupied to be transgressed; that a humid atmosphere is in all cases required as the vehicle of cholera, and that the prevailing wind directs its progress; that, nevertheless, cases of cholera occur by transmission from those who have been subject to the choleraic influence, or from fomites impregnated with the choleraic virus, but that no aggregate of cases so originated has ever produced a provincial manifestation of cholera."

Dr. De Renzy argues that cholera contagium is propagated chiefly by means of contaminated water. In this opinion he agrees with Dr. C. Macnamara, who states that "if we can only establish the principle that nothing but freshly and properly filtered water shall be consumed by the inhabitants of a town, barrack, or house, not only when at home, but when at work; at all times, in fact, when cholera is abroad, we may, I believe, discard all and every other means of preservation. Cholera cannot possibly gain a hold on any country, village, or home protected in this way. Measures of hygiene beyond are, of course, most useful in many ways, but they will not stop cholera, or, in fact, control its extension one iota, if the means above indicated be neglected. If the water supply is

a matter of importance in a country like England, what must it be in the case of the drinking-water in the endemic area of cholera?" The reasoning of Dr. De Renzy, and the strong facts with which he enforces his arguments, certainly, to our mind, are very convincing. That foul water is a common proximate cause of cholera is a fact of which we have long since convinced ourselves from actual observation. If, then, water is a common medium of propagating cholera in these countries, how much more frequently must not the disease be communicated in this manner in India, where the water supplies are notoriously largely contaminated with effete animal matter?

Dr. De Renzy gives the following suggestions for the prevention of the spread of cholera. They are applicable in the case of other contagious diseases.

"1. It is advisable to keep cholera patients as much as possible separate from the healthy.

"2. The attendants of the sick should not be more numerous than is absolutely necessary.

"3. The sick should be kept in an open airy place. Confinement of the patient in a close ill-ventilated room tends to render the disease fatal, and makes the attendants more liable to catch the disease. In open airy rooms there is very little risk of infection, if the following precautions are observed.

"4. The vomit and stools of the patient should be received in a 'Gumla,' half full of dry earth, and care should be taken to prevent any of the discharges being scattered on the floor. If any of the discharges by accident fall on the floor, the spot should be at once scraped, if the floor is kutcha, and the scrapings placed in the 'Gumla' to be disposed of as directed in next para. Should the floor be pucca, the spot should at once be cleaned, and then some fresh or hot ashes sprinkled over it.

"5. As soon as the earth in the 'Gumla' is saturated with discharges, the 'Gumla' containing it should be placed over a brisk fire and thoroughly heated, until all vapour has ceased to rise from the earth for ten minutes. The earth should then be buried.

"6. Persons who are too careless to attend fully to the instructions contained in para 5, should at least be careful not to allow the discharges to be scattered about the floor or village, and to have the earth in which they are mixed buried two feet below the surface of the ground.

"7. Several 'Gumlas' should be prepared with earth, and placed in convenient positions to receive any discharges that come unexpectedly from the patient.

"8. All clothes soiled by the patient should be washed daily in boiling

water. On no account should they be folded up and placed aside until they have been boiled. If there are no means of boiling them, as soon as they have been taken from the patient, they should be hung up on a rope or a branch of a tree in the open air until they can be boiled.

"9. No clothes belonging to a cholera patient should on any account be washed near a tank or a well, or in the vicinity of a stream from which drinking water is obtained. The water required for washing the clothes should be carried to a distance of not less than 50 yards from the well, tank, or stream, and there used for washing the clothes.

"10. The earth soaked with the discharges of the patient should on no account be placed or buried anywhere in the vicinity of a well, tank, or stream.

"11. The attendant on a cholera patient should not eat or drink without washing his hands in pure water immediately before doing so. The attendant should be careful to wash his hands as often as he may have occasion to assist the patient while performing the offices of nature, and immediately after assisting him. Personal cleanliness is an indispensable safe-guard against cholera infection.

"12. The attendants at the burying or burning of a corpse deceased from cholera should be as few as possible. After the ceremony they should wash their hands and clothes, and persons generally before returning home, but not near a well, tank, or stream used for water supply.

"13. The house where a person lies sick of cholera should be fumigated daily with sulphur. During the process the doors of the house should be kept open so as to admit a free current of air. The quantity of sulphur burnt should be sufficient to cause the persons present in the room to cough a little, and should be added gradually to the flame until this effect is produced. Any clothes soiled by the patient, after being washed and dried, should be spread out or hung on a rope and exposed to the sulphur fumes.

"14. The room in which a person may have died or recovered from an attack of cholera should be fumigated with dense suffocating fumes of sulphur, the doors and windows of the apartment being closed during the process for the space of two hours to confine the vapour. The room will have, of course, to be evacuated while the fumigation is going on. The 'charpoy,' clothes, and bedding of the patient, after being first well washed with boiling water and dried, should be so placed as to be thoroughly exposed to the fumes of the sulphur. With regard to the 'Sootlee' (string) the safest plan would be to burn it.

"15. After the fumigation the floor and walls of the house should be leeped.

"16. No person should eat sweetmeats or any other food in the room where the corpse of a person deceased of cholera may be lying.

"17. When cholera prevails no person should eat raw fruits, or raw vegetables, without first carefully washing them with pure water.

"18. No attendant on a cholera patient should touch or prepare food intended for others without first washing his hands.

"19. Personal, domestic, and village cleanliness is the surest preventive of the spread of cholera.

"20. An intelligent Hakim or Native Doctor should be deputed to carry out the disinfecting arrangements, and as in times of epidemic it is impossible to be sure of the cause of death in every case, it will be prudent to fumigate every house in which a death may occur whatever may be the alleged cause.

"21. Every case of cholera should be reported without delay for the information of the Deputy Commissioner, who will make arrangements for supplying the most appropriate medicines.

"22. The people of a healthy village will act wisely in abstaining as much as possible from all communication with the people of an infected village. Persons coming from an infected village should invariably be required to wash their clothes and persons before admission to a healthy village.

"23. The last caution is—destroy all cholera discharges by fire without delay, as the poison from which the disease spreads resides in them."

SMALL-POX AND VACCINATION.

Since the date of our last report small-pox has assumed an epidemic form in several parts of the United Kingdom. London and Liverpool have suffered severely from this loathsome disease. The fatality of the malady has not diminished as compared with the proportion of deaths to cases in former outbreaks. The disease broke out in Belfast, Drogheda, Dublin, and a few other places in Ireland, but up to the present it has not spread to any extent. In Dublin there have been only two or three deaths. In almost every case of small-pox, the source of the disease was traced to Liverpool or some town where small-pox was epidemic.

The prophylactic virtues of vaccination have been clearly proved during the present epidemic of small-pox. In Dublin where the immense majority of the people is vaccinated, the disease does not spread. During former visitations of the disease when a much larger proportion of the population was unvaccinated, the number of cases was very large.

The prevalence of small-pox has naturally directed increased attention to the subject of vaccination. It now seems to be generally admitted that the operation of vaccination is not quite so

very simple as it was formerly supposed to be; on the contrary, it requires some skill and attention for its proper performance. There is hardly a doubt as to the frequent failure of vaccination; and we suspect that an immense number of persons who imagine they are under the protective influence of the Jennerian genius, are only under the influence of a delusion. It has been stated that the mortality from small-pox, when the patients are vaccinated, is only 1 per cent.; but we believe it is considerably greater, probably 6 or 7 per cent. It is, however, more than likely that in most of the cases of those vaccinated persons, who die from variola, the vaccine operation had not been properly performed. It is certainly incumbent upon all vaccinators, public and private, to satisfy themselves fully that their work has been successfully accomplished.

At a meeting of the Epidemiological Society (London), held on the 8th of March last, Dr. Whitmore stated that the result of a house-to-house visitation in Marylebone showed that 5 per cent. of the children were not vaccinated, and 7 per cent. were imperfectly vaccinated; and further, that the public vaccinators were chiefly responsible for the imperfect attempts.

Some years ago the majority of physicians regarded re-vaccination as an unnecessary operation; but the progress of enlightenment has shown that it is almost as necessary as primary vaccination. The propriety of re-vaccination has been discussed repeatedly in the journals, and by the Medical Societies, and we think the weight of medical opinion leans in favour of the operation. At the London Small-pox and Vaccination Hospital the protective value of vaccination and re-vaccination has been clearly proved during the present epidemic. From 29th September, 1870, to 6th March, 1871, 751 cases of small-pox were admitted. Of these, 618 or 82 per cent. were vaccinated; 9 per cent. of the vaccinated died, whilst 44 per cent. of the unvaccinated perished. Those amongst the so-called vaccinated, who had no characteristic marks, suffered severely, 22 per cent. succumbing to the malady. The nurses in the hospital were re-vaccinated, and not a death amongst them occurred. At Homerton Small-pox Hospital no re-vaccinated persons were admitted. At Hampstead the mortality amongst the vaccinated patients amounted to 7 per cent.; whilst 38·5 per cent. of the unvaccinated patients died. Dr. Grieve states in the *Lancet* for March 8, 1871, that in 800 cases of small-pox, which came under his notice in this hospital, there was not one which occurred after provable re-vaccination. As there has been a perfect vaccination

mania in London and elsewhere, it is evident that a large proportion of the inhabitants of Hampstead must have undergone that operation.

At a meeting of the Surgical Society of Ireland, held on the 31st of March and 21st of April, 1871, the subject of re-vaccination was fully discussed: the majority of the speakers expressed on the whole an unfavourable opinion relative to the repetition of vaccination; but to our mind the arguments in favour of re-vaccination, urged by Dr. G. H. Kidd, appeared very convincing. Dr. Kidd argued that a large proportion of those who were vaccinated in youth become liable to suffer from small-pox in after life. A proportion as high as 6 per cent under ordinary epidemic influence as proved by the epidemic at Marseilles, recorded by Bousquet, and that at Newark-on-Trent, recorded by Bigsby, and under circumstances of repeated exposure or concentrated epidemic influence, as high even as 12 per cent. or more. That this was due to the wearing out of the vaccine influence, Dr. Kidd argued, was proved by a comparison of the ages at which the vaccinated and unvaccinated take small-pox. The greatest number of the unvaccinated taking the disease before ten years of age, while the vaccinated do not take it till between ten and twenty-five years of age, showing that vaccination in infancy is an efficient protection up till ten years of age, and then begins to wear out. This wearing out of the vaccine influence, Dr. Kidd further argued, was also shown by the gradual prolongation of the stages of incubation and development in cases of re-vaccination; and he concluded by stating his opinion that re-vaccination should in all cases be performed between the ages of 10 and 15 years, and be repeated, under circumstances of peculiar exposure to small-pox; and that when no effect follows the operation, it should always be repeated.

Dr. Mapother contended, on physiological grounds, that a disease induced in an infant a few months old, would hardly produce effects which would last unimpaired during a lifetime.

We believe that the prophylactic virtues of vaccination gradually die out; and we think that the operation should be repeated immediately after puberty. It would, indeed, be perhaps desirable to re-vaccinate periodically, say every 10 or 15 years.

A Parliamentary Committee on Vaccination has examined a number of witnesses for and against compulsory vaccination. Much of the committee's time was uselessly occupied in listening to what well might be termed the ravings of the anti-vaccinators. On the

other hand, the strongest evidence in favour of vaccination was given by Mr. Simon and other gentlemen, who clearly exposed the fallacious reasoning, and one-sided statistics of the members of the "Anti-vaccination League."

Mr. George S. Gibbs, whose translation of the report of the French Academy of Medicine, respecting vaccination in France, we noticed in our last issue, has written to point out a passage in our critique, which he deems objectionable. We wrote:—"In the ten departments where Mr. Gibbs says there were fewest vaccinations, and the fewest cases of small-pox, the following figures apply":—We then quoted the figures for the first ten departments from Table II., annexed to the report, showing an average proportion of vaccinations to births of 22·7 per cent.; of small-pox cases to births of 0·404 per cent.; of small-pox deaths to births of 0·096 per cent. We then proceeded to "compare with these figures the statistics of ten of the departments where vaccinations were most practised," and quoted a selection from the last twenty-one departments, specified in Table II., annexed to the report, showing an average proportion of vaccinations to births of 92·7 per cent.; of small-pox cases to births of 0·202 per cent.; of small-pox deaths to births of 0·015 per cent.

Mr. Gibbs is of opinion that the selection of ten sets of figures from twenty-one, to compare with ten sets of figures taken *seriatim*, is a mode of criticism calculated to mislead any person who had not all the figures before him; and he has forwarded to us the subjoined tabulated statement of ten departments, selected from the first nineteen departments mentioned in Table II., these being where vaccinations were least practised, showing an average proportion of vaccinations to births of 27·1 per cent., of small-pox cases to births of 0·172 per cent., of small-pox deaths to births of 0·014 per cent.

Mr. Gibbs also points out that by adopting a different principle of selection from the figures at either end of the table equally, or proportionately, high percentages might be obtained.

Lastly, Mr. Gibbs objects to being represented as the author of the document translated by him, and "abridged" merely by the omission of the unimportant and tedious details. He considers himself responsible solely for the accurate transcription of the figures given in the original, and for the many calculations necessary to show their statistical scientific and social value. He, of himself, "says" nothing; and he believes that his reading of the several tables is perfectly correct.

Department	Percentage of Vaccination to Births	Percentage of Small-pox Cases to Births	Percentage of Small-pox Deaths to Births
Creuze - - -	16	0·10	0·00
Aveyron - - -	19	0·00	0·00
Ardennes - - -	20	0·00	0·00
Chir - - - -	21	0·35	0·02
Loiret - - -	25	0·37	0·02
Mauche - - -	28	0·00	0·00
Sarthe - - -	31	0·33	0·02
Pyrenees Hautes - -	32	0·19	0·04
Landes - - -	36	0·19	0·02
Loire Hautes - -	43	0·19	0·02
Average - -	27·1	0·172	0·014

The total number of births in these ten departments was 88,287.

We willingly comply with Mr. Gibbs' request to place the figures of the pamphlet, which he has translated, in the way in which he considers they should be exhibited, but, at the same time, we cannot agree with him that our readers were misled by the manner in which we gave the statistics contained in this pamphlet. We gave, without alteration, the two most important tables, in which all the important facts were epitomized. The pamphlet is evidently intended to prove that where vaccination is most practised, small-pox is most prevalent. If that be the case, why is it that in some of the departments where there were few vaccinations, there was a large mortality from variola, and *vice versa?* It is noticeable that in Alpes Maritimes, where the vaccinations were 25 per cent. of the births the number of small-pox cases amounted to 1·97 per cent. of the births; whilst, on the other hand, in Corrieze, where the vaccinations and re-vaccinations were in the ratio of 110 per 100 births, the cases of small-pox were only 0·08 per cent., and of deaths from the disease 0·01 per cent. It happened that in a very small number of the departments "most vaccinated" there were outbreak of small-pox; and the statistics of these departments unduly influence those of all the other "most vaccinated" ones.

We notice that the total number of deaths from small-pox in the whole of France was very small. 1,652 persons died from small-pox in 1867; the births amounted to 700,000, and the deaths (the

population of France is increasing very slowly) were probably about equal. Now, the number of 1,652 deaths from variola in a population of 37,000,000 is comparatively very small, and it does not admit of any positive conclusions being deduced from it. In 6 of the 89 departments the deaths from small-pox amounted to 1,080. We have totted up the numbers of deaths from small-pox which occurred in a single year, as shown in Table I. of Mr. Gibbs' translation; they amount to 2,089. Of these deaths 1,080 *or more than one-half occurred in* 6 *out of* 89 *departments.* Nearly the same number of deaths from small-pox occurred in a single fortnight during the late outbreak of the disease in Paris.

In an article upon the vitality of vaccine virus, contributed by M. Melsens to the *Journal de Pharmacie et de Chimie* for September, 1870, the results of the action of cold upon the virus are described. It is stated that the virus retained its activity after being exposed to the intense cold of 80 *below* zero on the centigrade scale. As heat exercises an injurious influence upon vaccine lymph, it might be found practicable in sending lymph to tropical climates (as is sometimes done), to enclose it, properly protected, in ice.

CHARBON IN THE HUMAN SUBJECT.

The disease termed *charbon*, or malignant vesicle, occasionally attacks oxen in Ireland. We have seen a few cases of it, but it is much more common amongst cattle and horses in France and other Continental States. It is stated that the flesh of animals affected with this disease has often been eaten with impunity; but, on the other hand, it has been alleged that persons have died from eating this kind of diseased meat. Butchers who prepare, or "dress" the carcasses of animals affected with charbon are liable to contract the disease. About three years ago a butcher near Cork lost his life by allowing the matter from the vesicles of a cow affected with charbon to come in contact with his skin.

In the last Report of the State Board of Health of Massachusetts an account is given of a number of deaths from charbon which took place under peculiar circumstances. For several years past, and up to November, 1870, men employed in a factory at Walpole, Massachusetts, died from charbon. Since March, 1866, there have been 26 cases of the disease, of which 14 terminated fatally. Dr. Stone shows that the disease was conveyed into the factory in horse hair imported from South America. Trousseau

states[a] that in two French factories where horse hair from South America was employed as raw material, 20 deaths from charbon occurred in 10 years, although the average number of persons employed was only from six to eight. We have no doubt but that hides, hair, and especially old clothes, are common carriers of various kinds of contagion. We have known a case where a feather bed on which a cholera patient expired was promptly sold to a person who, the day after he slept upon it, caught cholera and died, and the bed was disposed of to a third person, whose fate we know not. This fact was verified before the Public Health Committee of the Corporation of Dublin. Cholera may not be a disease readily transmitted through the medium of clothing, but that scarlatina and some other zymotics are often conveyed in that way there is very little doubt.

CORRELATION OF ZYMOTIC DISEASES.

Mr. A. Wolff, F.R.C.S., in a recently published pamphlet, expresses his dissatisfaction with the existing theories in relation to the intimate nature of contagious diseases. He contends that there is not a multiplicity of zymotic poisons, but that all diseases are molecular motions of varying degrees of intensity. A dissection wound is one of the best illustrations of zymotic action in its simplest form, "there being no question of a special germ or vitiated secretion; so also it is observed when particles of textures which are themselves inflamed become capable of secreting inflammation in particles with which they have contact." Dr. Wolff is of opinion that all the phenomena of the zymotic diseases are under the control of the ordinary physical laws which govern growth and decomposition.

He arranges the zymotics into groups, the diseases in which are connected by points of resemblance—for example, variola, vaccine and varicella, measles and scarlatina, and so on. A number of persons placed together under insanitary conditions, and exposed to the influence of decomposing animal matter, will not always become affected with the same zymotic; for the precise form which the disease may assume does not depend upon the presence of a specific germ, or entity, but upon a variety of causes, such as, for example, the channel through which the *materies morbi* comes in contact with the healthy subject.

Notwithstanding the very ingenious reasoning of Dr. Wolff, he

[a] Gazette Medicale, February, 1847. No. 4.

has not satisfied us that the cause of cholera is to be found in the catalysis of ordinary animal matter. As truly might it be said that all the parasitic maladies which affect animals are referable to the attacks of a single creature as to assert that cholera and small-pox have a common origin. When a zymotic disease is prevalent, who can doubt but that it is produced by a special and not by a general cause? Simple decomposing animal matter may produce some form of disease, but could it, under any circumstances, develop syphilis, or small-pox, or rabies?

There is no doubt as to the epidemic nature of typhoid fever, and its contagious character is unquestionable; but the cases where it is directly communicated from the sick to the healthy appear to be very rare. We are disposed to regard this zymotic as one which does not admit of being extirpated by human agency. We believe that if all the poison of small-pox, scarlatina, measles, and syphilis now in existence were destroyed, these diseases would probably never again afflict humanity. By means of an extraordinary combination or series of combinations of morbific influences, other zymotic diseases might take their place; but new diseases are just as likely to come into existence whether or not small-pox, scarlatina, *et hoc genus omne*, are removed from the list of ills which flesh is heir to. When our knowledge of the etiology of zymotic diseases is more perfect than it is, we shall probably distribute these maladies into two groups—namely, one to include all diseases which are only directly or indirectly communicated from individual to individual—syphilis and small-pox, for example; one, in which to place those diseases which, though contagious, are sporadic under certain conditions of soil, climate, vegetation, &c. Miasmatic fevers, enteric fever, and cholera morbus are diseases which, in all probability, originate from time to time spontaneously.

DEATH AND SICKNESS REGISTRATION.

The vital statistics of the Irish people, which have been published since the Registration of Births, Deaths, and Marriages Act came into operation, are notoriously unreliable. A large sum of money is annually expended in collecting vital statistics, which, with perhaps the exception of these obtained in Dublin and a few other towns, are wholly unreliable. Undoubtedly it would be better either to abandon the system of births, deaths, and marriages registration, or to make a vigorous attempt to improve it. We hope the latter alternative will be adopted, and that immediate steps

will be taken to secure accurate and full returns of the deaths, births, and marriages, which take place in this country. There are no insuperable difficulties in the way—all that is required involves only a slight increased expenditure of money, and the adoption of more stringent regulations.

In the second Report (Vol. iii.) of the Royal Sanitary Commission just published, we find that the Commission heard evidence relative to death registration in Ireland. Dr. W. M. Burke, the medical superintendent of statistics gave very strong testimony against the present system of registration, which, especially in the country districts, was "very imperfect." Dr. Burke stated that the form of death register (which is prescribed in the Act applicable to Ireland) is objected to by medical men, because in signing it they testify to the actual death, which in most cases they do not witness, or of which they have no ocular proof. The certificate to be signed by the medical attendant should merely state the maladies under which the deceased laboured when last seen by the medical attendant. Dr. Burke stated that medical men "very often" refuse to sign death certificates. In reply to a question put to him, he said "medical men object more to the wording of the certificate than anything else. I think I can illustrate this by a circumstance which occurred in Dublin ; I cannot fix the date of the occurrence, but it is not very long since; a physician, in large practice in Dublin, was told that a patient of his was dead. The physician, on being informed of his death, went up to the patient's room and found him alive. The certificate goes on to say, 'I hereby certify that I attended' so-and-so, 'who was apparently aged, or was stated to be aged,' so many years. 'That I last saw him on' such a date, "and that he died on a certain day; that the cause of his death was' so-and-so; 'and that the disease had continued for' a certain period. The physicians and surgeons of Ireland object to being asked to furnish all those details, and to furnish them gratuitously. In the majority of cases they say that they may not have been present when the patient died; and all the evidence they have of the patient's death is the information which they receive from some of the family, or other persons. They are, generally speaking, willing and anxious to certify as to the cause of death, but they object to be informants of the death."

One—and the chief—cause of the defect in the registration of births and deaths in Ireland is the negligence of the people. There should be a heavy penalty imposed upon those whose duty it is to

register those events. Dr. Burke said that the services of the police could be rendered available in ascertaining the occurrence of births and deaths. These events could hardly take place in the rural districts without coming to the knowledge of the police, if the latter had a special reason for acquiring such information. The special reason should be a *douceur* in the shape of a shilling fee. We believe that if the services of the police were enlisted in this matter, the registration of vital statistics in Ireland would, so far as ascertaining the number of births and deaths is concerned, soon approach perfection.

It is evident that the birth-rate amongst a population cannot be actually determined if the still-born children are not taken into account. The births of still-born children are not registered in Ireland; and Dr. Burke stated that it would be impracticable to do so: it would, however, be most desirable to make their registration compulsory, for in such case a large proportion of them would be recorded in the registrar's lists.

With respect to disease registration, Dr. Burke was of opinion that practitioners would not, and should not, afford information relative to the diseases from which their private patients suffered. He thought the statistics of disease could be sufficiently collected from hospitals, dispensaries, work-houses, lunatic asylums, jails, and public institutions. The statistics obtained from these sources would, according to Dr. Burke, give a fair account of disease generally in the country. As practitioners constantly publish cases of disease occurring amongst their private patients suppressing, of course, their names, they might probably see no impropriety in furnishing to the registrars, periodically, *confidential* statements (omitting names), showing the number of cases of particular disease which they had treated during a particular time, and within certain areas.

At a meeting of the Medical Society of the King and Queen's College of Physicians, held on the 15th March, 1871, Sir Dominic Corrigan read a paper on "Death Registration, and Medical Certificates of Death." The author condemned the form of death certificate at present in use; and he stated that he refused to sign death certificates, for only once during the last eight years, which have occurred since the passing of the Registration Act, was he able to testify from actual personal knowledge to the time at which the death of a patient occurred.

"Let," says Sir Dominic, "the Legislature mend its ways and

correct the present defective state of the law without expecting the medical profession to sacrifice truth and self-respect to prop blundering Acts of Parliament.

I hope this state of things will soon be amended. A revised code of sanitary laws is almost now under consideration, and I trust our Legislators will learn that, in framing such laws, they should seek for assistance from those bodies that understand the subject."

PART IV.

MEDICAL MISCELLANY.

Reports, Translations, and Scientific Intelligence.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

Dr. J. Stannus Hughes, President.

Epithelioma.—Dr. A. W. Foot exhibited an epithelial growth, removed from the side of the head of a golden eagle, in the Dublin Zoological Gardens, on the 4th of April. The growth originated from a bite of a rat, after six weeks. Its structure appeared to be hypertrophy of the cere, and was of a semi-corneous consistence; on section, the exposed surface was found to present a warty appearance. After the removal of the tumour, some warty growths near its base were excised, and the hæmorrhage checked by the free application of strong nitric acid, which was used with the view of preventing, if possible, any recurrence of the growth, and with the object of causing such contraction in the wound as would brace up the loose skin so as to hide the cicatrix. The nitric acid seemed to cause the bird a good deal of pain, which it bore with the fortitude so characteristic of these noble birds of prey. On the 8th the wound had quite healed, and the bird's comfort and appearance were visibly improved.

The tumour, which looked before removal uncommonly like the warty ulcer of Marjolin, may fairly be classed as an epithelial growth, inasmuch as the cere of an outgrowth of which it was principally composed, is one of the typical examples of epithelial structure.—*April 9th*, 1870.

Polypus voided from the Rectum.—Dr. H. Kennedy exhibited a small tumour, which had been expelled from the rectum of a gentleman in perfect health, and for which he was indebted to Dr. Brown, of the Cork-street Hospital. The patient never suffered any local distress, except when the bowels acted, when he had a sense of uneasiness; he had not had piles, or bleeding from the rectum. On one occasion he expelled this small tumour, which had all the appearance of a polypus. The peculiarity about it was, that it came away spontaneously. The gentleman, who was twenty-three years of age, had, since the expulsion of this body,

remained perfectly free from the suffering, which used to affect him after each alvine discharge.

The tumour, when expelled, was as large as a raspberry, of a fleshy appearance, and firm to the touch. From its being single, the great probability is, that it was a polypus.—*April 9th*, 1870.

Pulmonary Emphysema.—Dr. Hayden said the specimen which he had to bring under the notice of the Society was an example of a not very common form of disease—viz., circumscribed, vicarious emphysema. The patient, from whose body it was obtained, was a man, aged thirty-three, of intemperate habits, and scrofulous diathesis. He was employed as a carter, in the service of one of the railway companies in this city. He first came under his notice last August. He was then suffering from pneumonia, consequent on, if not caused by, a fall from his cart, while under the influence of liquor. From the 20th of August to the 11th of September he remained under his care, and left the hospital well. He returned, however, to his intemperate habits; and he saw him again on the 29th of last month, when he was admitted a second time. He was then suffering from extreme dyspnœa. His face was congested, and he coughed and expectorated freely. He lived from the 29th of March to the 3rd of April, and died asphyxiated on that day.

The condition of his chest was as follows:—It was resonant throughout in front, and loud râles were audible over the anterior surface of the chest. Posteriorly resonance was normal, and râles were audible at both sides; but over the extreme base of the right lung posteriorly, there was exaggerated, amounting almost to tympanitic resonance. The respiratory sound was almost abolished, and the râles were confined to the acme of inspiration; and at this moment there was likewise audible a metallic click, of a well-pronounced character, such as may be heard in pneumothorax with effusion. The question then arose, whether, considering the man's habits and constitution, it was not a case of pneumothorax with effusion? He came to the conclusion that it was a case of excessive dilatation of the air cells, or, in other words, of circumscribed emphysema; and for these reasons:—There was no dulness associated with it; though the metallic tinkling was so distinct, there was no hippocratic splashing, or amphoric breathing, no displacement of organs, and no abolition of vocal fremitus.

On examining the body after death, they found the lungs in the following condition:—The apex of the left lung was condensed and puckered by cicatrization of an old tubercular cavity, whilst the anterior margin and the base presented a condition of exaggerated emphysema. It was not easy to say whether this was sub-pleural or intra-vesicular; for he thought, with Dr. Stokes, that the test of being able to displace the air by finger pressure was not infallible in aggravated cases like this. In the

base was likewise found a solid mass, which, on section, presented all the evidence of having been the result of an unresolved pneumonia. The right lung was in the same condition as the left; at the base and anterior edge, the organ presented the appearance of large air-filled cysts, of a size varying from that of a marble to that of an orange, projecting from the surface, and within these the air was partially movable by pressure; the apex on this side was likewise emphysematous. In the base of this lung also there were solid nodules, but not so large as those in the base of the left lung. Dr. Hayden cut into one of these masses, and found it contained pus, and presented very much the appearance of a pyæmic abscess, with surrounding solid tissue; and he doubted not that, if this man had been the subject of a surgical disease or operation, such would have been the explanation given. There could be no doubt, however, that the emphysema was compensatory, or vicarious, consequent on the destruction, so far as breathing was concerned, of the other parts of the organ. The heart and other organs were sound.

He would call attention to the possibility arising from the physical sign of metallic tinkling, of confounding pneumothorax with circumscribed, exaggerated emphysema. The diagnosis, however, was, he thought, not at all difficult. In the first place, in the case now under notice, in addition to the metallic click, a feeble respiratory sound, and bronchial râles were likewise heard; and, as already mentioned, there was no fistular breathing or splashing, and vocal thrill was perceptible.

The metallic click was manifestly the result of a movement of liquid within the exaggerated air vesicles during forced respiration—*April 9th*, 1870.

Mammary Tumour.—Dr. Tyrrell exhibited an example of chronic mammary tumour which he removed from the breast of a woman admitted to the Mater Misericordiæ Hospital some months since. The breast was extremely pendulous, very much enlarged, slightly discoloured on the inferior portion, and having some veins ramifying over it. At first sight one might be disposed to think it was malignant, but on close examination and getting a history of the case he came to the conclusion that it was not so. In the first place it was very seldom that a malignant tumour had this pendulous form, and again, the glands were entirely free from disease, and the growth of the tumour was painless and slow, having taken three years to attain the size which it had reached at the time of operation. Again, on examining the tumour more carefully, it was found to have a peculiar elastic feel; it was slightly nodulated on the surface, the nipple was not at all retracted, and it was quite movable. The operation was an extremely easy one—merely making two elliptical incisions and removing the breast with the tumour attached. The case went on favourably.

On section it presented the appearances to well described by Brodie, and also by Mr. Paget.

These tumours were rather rare. He had had twelve cases of tumour of the breast under his care in the Mater Misericordiæ Hospital during the year; eleven of them were malignant, the present being the only case of non-malignant tumour of the breast that had come into the hospital under his care. Of the eleven cases of a malignant nature, the breast was removed in six, and five of the patieuts were sent home again, their cases not being considered suitable for operation. In the present case they were able to give a favourable prognosis. It was remarkable that at the time the breast was removed the woman was suckling a child, and in the portion of the breast above the tumour there was milk. He would not have removed the tumour until the child had been weaned but for the urgent entreaties of the woman, who had come up from the country to have the operation performed.—*December* 17*th*, 1870.

Aneurism of the Aorta.—Mr. Hamilton exhibited an example of aneurism of the arch of the aorta. The subject of the disease was a pensioner, thirty-nine years of age, who was admitted to the Richmond Hospital with an aneurismal tumour on the right side of the neck. It extended from above the sterno-clavicular articulation to a little below the bifurcation of the carotid artery. When he first saw the patient, he was sitting up in bed, gasping for breath. His face was purple, his eyes were staring and all the veins of his neck were enlarged, particularly the external jugular, which was contorted, and distended to the size of the little finger —in short, his appearance was that of a man about to suffocate. His head was bent forward on the chest, to relieve the trachea from the pressure of the aneurism. There was a tumour, which pulsated strongly, about the size of an egg, on the right side of the neck, above the sterno-clavicular articulation, and bulging forwards and inwards towards the mesian line. It extended below the level of the sterno-clavicular articulation and the upper end of the sternum. It had a loud double sound, but no *brûit ;* and this double sound was traced into the chest, behind the upper end of the sternum, and down to the arch of the aorta, growing feebler as it approached the heart. The action of the heart was feeble, and the sounds not very distinct; the left radial pulse was distinctly weaker than the right. The history he gave of his case was as follows: —Eight months previously, when lifting a heavy weight, he suddenly felt an acute pain in the region of the sterno-clavicular articulation, as if something had given way. It was some time before any tumour was observed. He then noticed one in the lower part of the neck, which increased in size, but gave him little annoyance till five weeks before his admission, when he suffered from great difficulty of breathing. After being in hospital two days, he got such a paroxysm of dyspnœa, that the

resident pupil, who was called up to see him, thought he would die. It became an interesting question, what vessel was the seat of the aneurism? The first and most obvious conclusion was that it was an aneurism of the carotid artery, and this was the opinion of the gentleman who sent the case into the hospital. He (Mr. Hamilton) had his doubts on the point, and for the following reasons:—In first place the carotid artery, above the aneurism was healthy; secondly, he could not get his finger below the tumour. When he tried to follow it into the chest, he was unable to do so; and also the fact of the double *brûit* extending down into the chest behind the sterno-clavicular articulation led him to believe that it was not a carotid aneurism. Was it, then, an aneurism of the arteria innominata? This was more likely; but on carefully considering the case, he did not think so. There was not that dulness on percussion which he had invariably heard at the sternal end of the sub-clavicular region, when the arteria innominata was affected by aneurism. Moreover, the right carotid and sub-clavian were both normal. If it had been an aneurism of the arteria innominata, this would scarcely have been the case; these vessels were generally more or less affected under such circumstances, and the pulse in the right wrist was usually affected. A further reason was, that the man was suffering great dyspnœa from pressure on the trachea at its bifurcation; whereas, in cases of aneurism of the innominata (unless where the tumour was very large, which was not so in this case), the pressure was generally on the right bronchus; and they had on the right side of the chest morbid sounds, or an absence of respiratory sounds, with clearness on percussion. This absence of dulness on percussion, therefore, led him to believe it was not an aneurism of the arteria innominata. He, therefore, came to the conclusion that it was an aneurism of the arch of the aorta at its upper and middle portion, from its pressing on the bifurcation of the trachea; and he thought that from the small extent laterally of the double sound, the neck of the aneurism was narrow. Burns and others had described cases of aneurism of the arch of the aorta so narrow at their origin and so large above that they had been likened to a pear. Burns compared it to an oil flask with the narrow neck downwards. On the third day after admission the man had another severe paroxysm of dyspnœa, and by a slow process of suffocation he died.

They could now see the aneurism springing by a narrow neck from the arch of the aorta, between the arteria innominata and the left carotid artery; narrow below, it gradually expanded. It was filled with solid coagula, particularly at the back, but in front the sac was exceedingly thin. By its compression it had obliterated the left vena innominata, and in the right ventricle was a firm embolus, extending from the ventricle up the pulmonary artery. The lungs were emphysematous.—*April 9th*, 1870.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-FIRST ANNUAL SESSION.

Dr. Kidd, President.

Dr. John A. Byrne said that although polypi of the uterus and fibrous tumours were by no means of rare occurrence, and the Society had often had cases brought before it by its members, and he need scarcely remind them of the very interesting specimen of large tumour which was exhibited last session by Dr. Atthill, and some also which he had himself exhibited; yet tumours of the vagina are acknowledged by all writers upon gynecology to be extremely rare. Cases, however, are recorded by different writers, and he would refer the members of the Society to the works of Scanzoni, Kiwisch, Paget, and M'Clintock for details of many interesting cases of those neoplasms. Dr. M'Clintock's work is rich indeed in recording many cases of this affection as well as of cystic tumours of the vagina.

He had lately met with an important example of the former variety of tumour, the history of which, with the permission of the Society, he would read. He regretted that he was unable to exhibit the specimen, as it had been unfortunately lost.

Mrs. K., a lady, aged thirty-six, the mother of four children, consulted him in the autumn of 1869, on account of a tumour which she had for some time felt protruding through the vulva.

On making a vaginal examination he felt a soft firm tumour of about the size of a hen egg, pale in colour, quite movable, and free, and capable of being pulled downwards or replaced upwards in the vagina. Coughing, or forced expiration, also pushed it outwards. It was attached to the posterior wall of the vagina, within an inch or inch and half of the vulva. The mode of attachment was not by pedicle, as the polypi which we see so often growing from the walls of the uterus, but it appeared to be folded up in a large loose fold of the mucous membrane of the vagina, which had become very elongated in consequence of the tumour stretching it, and pulling it downwards by its weight. The os uteri and uterus were felt above, and quite distinct from it. The history which she gave was, that she had always enjoyed excellent health, had never suffered from any inconvenience until she became pregnant with her fourth child, when she observed some fulness, and felt a sensation of bearing down. She consulted a medical man in the country, who told her it was a polypus. When she was confined those symptoms abated considerably, and during her confinement it did not appear to interfere with the labour

in any way, which was as easy as usual. She then became again pregnant, and when about five months or so began to experience considerable annoyance from the bearing down, and irritation caused by the tumour. She then came up to Dublin for advice.

It was at this period that he first saw her, and had an opportunity of examining it.

He explained to her the nature of the case, and recommended her to defer anyo perative interference until after her confinement, which advice she followed, and about six months after her confinement, which was as easy as the previous, she came back to Dublin and placed herself under his care.

Dr. Kidd saw her in consultation with him, and they both agreed in the opinion that it should be removed.

Operation, October 1st.—Having undergone some preliminary treatment he proceeded to remove it.

She was placed on a table opposite a window in the lithotomy position, and chloroform was exhibited; this she bore very badly, vomiting set in and symptoms of collapse, and they were obliged to desist from its exhibition.

Dr. Kidd having separated the labia for him and elevated the tumour towards the pubis, Dr. Byrne introduced his left index finger into the rectum. On doing so the point of the finger passed into a *cul-de-sac* behind the tumour, and it became manifest that there was only a slight partition between the tumour and rectum; so that it made him very careful in not cutting too deeply. A V-shaped incision through the mucous membrane with the point of the V downwards having been first made, the tumour was dissected carefully out of the cellular tissue in which it lay. It required much time and a great deal of care on account of the great number of small vessels, chiefly veins, which lay imbedded between the rectum and the posterior surface of the tumour. Moreover, the partition was so thin between it and the interior of the rectum that it was with the greatest difficulty he avoided wounding this important organ. A few small arteries which spouted were tied, and a few twisted, and the bleeding arrested. Having at length dissected out the tumour he passed three sutures through the edges of the triangular wound made by the incision, and drew them together.

The incision, or rather excision, on allowing the parts to remain *in situ* for a moment after the removal, was tolerably large, and a raw surface appeared in the site of, and extending below the site of the tumour along the inner surface of the labiæ. Hence in order to guard against any prolapse of the rectal wall of the vagina in future, he determined on uniting the inferior portion of the labiæ by quill suture. Three sutures were passed through this part of the new surface and firmly tied on each

side over a quill-shaped piece of gum elastic. Thus every precaution was taken against hæmorrhage and prolapse.

She lost a considerable quantity of blood during the removal; a fact which should be borne in mind by all undertaking operations in this locality, and which should be guarded against by every means in the surgeon's power.

She was removed to bed, lint dipped in cold spirits and water was applied, and some brandy and water were given.

He saw her again in a few hours, the vomiting produced by the exhibition of the chloroform, had unfortunately again been troubling her; she was very distressed, and she was very low and weak. On looking at the vulva he observed that she had lost some blood, and that she was still loosing some. He syringed her with cold water, and applied lint saturated with perchloride of iron to the part. Burnt brandy and ice were exhibited, and prussic acid and sinapisms were applied to the epigastrium, and thus in some time this disagreeable symptom was subdued. The intense sickness caused her to loose a considerable quantity of blood; by the frequent application of the perchloride, passed into the vagina on cotton, it was arrested.

An opiate was exhibited, the catheter was passed occasionally, a suppository was introduced into the rectum as required, and she began to rally towards the morning.

The quill sutures were removed on the seventh day, and the small wire sutures on the ninth day.

They did not produce any annoyance whatever, and the result of their application was most satisfactory. The lower part of the vulval aperture was united for a short distance, and the union of the incision in the posterior wall was most complete.

It would be troublesome to the Society to detail the treatment, suffice it to say that the catheter was passed twice daily for some days; opiate suppositories were employed to keep the bowels quiet until union had taken place, and good nourishment given.

The lady recovered perfectly; there was no rectal prolapse. She has since returned to the country, and he has received a letter from her lately in which she informed him that she has been regular, and that she experiences much comfort and no inconvenience from the removal of the tumour.

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